ISIS

International Review devoted to the History
:: of Science and Civilization ::
Quarterly Organ of the History of Science Society and of the International Committee of the History of Science.

SUMMARY
of No 60 (Vol. XXI, 1)
230 p., 10 pl., 8 fig.

I. — *Section of the History of Science of the Carnegie Institution. 15th annual report* 5

II. — *Main articles* (13)

G. SARTON *(Cambridge, Mass.)*.—The artificial fertilization of date-palms in the time of ASHUR-NASIR-PAL, B.C. 885-860 (4 pl.) 8

J. RUSKA *(Berlin)*.—Die Alchemie des AVICENNA 14

M. NIERENSTEIN and F. M. PRICE *(Bristol)*.—The identity of the MS entitled "Mr. Nortons worke, *de lapide ph'orum*" with the Ordinall of Alchimy (1 pl.) 52

S. V. LARKEY & L. TUM SUDEN *(San Francisco)*.—JACKSON's English translation of BERENGARIUS OF CARPI's "Isagogae Breves," 1660 and 1664 (5 fig.) 57

T. COWLES *(Berkeley)*.—Dr. HENRY POWER's poem on the Microscope 71

W. C. WALKER *(London)*.—The beginnings of the scientific career of JOSEPH PRIESTLEY (4 pl.) 81

N. E. STEVENS *(Washington)*.—Plant pathology in the penultimate century 98

M. NIERENSTEIN *(Bristol)*.—A missing chapter in the history of organic chemistry: the link between elementary analysis by dry-distillation and combustion (2 fig.) 123

R. A. SAMPSON *(Edinburgh)* & F. E. BRASCH *(Washington)*.—JOHN LOUIS EMIL DREYER (1852-1926) (portrait) 131
L. THORNDIKE *(New York City)*.—Check-list of rotographs in the history of natural and occult science. 145
A. C. BURR *(Cambridge, Mass.)*.—Notes on the history of the experimental determination of the thermal conductivity of gases 169
E. FÄRBER *(Heidelberg)*.—Stoff und Form als Problem der biochemischen Forschung. Eine geschichtliche Betrachtung . . 187
J. R. PARTINGTON *(London)*.—The discovery of Mosaic Gold . 203

III. — *Notes and correspondence* (5)

Queries.—JEAN TRENCHANT, French mathematician of the 2nd half of the 16th century (1 fig.) (G. S.).—ADRIAAN VAN ROOMEN's commentary on AL-KHWĀRIZMĪ (c. 1598) (G. S.).—Primitive tonsillotomy (G. S.) 207
Miscellanea.—Cours et conférences de l'Institut d'histoire des sciences, Paris, 1933-34.—Prix Binoux 1933 (L. G.) . . . 210

Reviews (8)

XVth Century. NICOLAE DE CUSA *De Docta Ignorantia libri tres*. Ed. E. HOFFMAN et R. KLIBANSKY. Leipzig, MEINER, 1932 (S. KURLAND) 211
XVIIth Century. CHRISTIAAN HUYGENS. Œuvres complètes publiées par la Société hollandaise des sciences (T. 1-17). La Haye, NIJHOFF, 1888-1932 (à continuer) (G. S.) 213
DOBELL, C. ANTONY VAN LEEUWENHOEK and his " Little Animals." New York, HARCOURT, BRACE, 1932 (C. A. KOFOID) 216
XVIIIth Century. ZIRKLE, C. Some forgotten records of hybridization and sex in plants, 1716-1739. Washington, Journal of heredity, 1932 (G. S.) 218
Greece. HEIDEL, W. A. The heroic age of science. Baltimore, WILLIAMS & WILKINS, 1933 (G. S.) 220
REY, A. La jeunesse de la science grecque. Paris, La Renaissance du livre, 1933 (G. S.) 224
Islam. The MACDONALD presentation volume. Princeton, University press, 1933 (G. S.) 226
Physics. CLAY, R. S. & COURT, T. H. The history of the microscope. London, GRIFFIN, 1932 (C. A. KOFOID) 227

Section of the History of Science of the Carnegie Institution

Fifteenth annual report, for the period extending from July 1, 1932 *to June* 30, 1933 *(Previous reports appeared in Year Books no.* 18 *to* 31*; the* 12*th and following also appeared in* Isis, *the latest in vol.* 19, 15-18*).*

1. Dr. SARTON returned to Cambridge on September 25, 1932 after an absence of almost fourteen months, the purpose of which has been discussed in the previous report. After his return he was obliged to spend considerable time in catching up with the work done by various scholars while he was away, and in studying a large number of publications which had accumulated in his office. He took pains to coordinate his observations and reminiscences, while these were still fresh, and to integrate them into the rest of his knowledge.

Having been honored by the University of California with the Hitchcock professorship he spent five weeks in Berkeley (Jan. 4 to Feb. 7) and delivered there eight lectures on Medieval science and medieval culture (with special reference to the Mediterranean world and the Arabic writings). These lectures were his first attempt to coordinate the results of his studies and of his travels. He will prepare them for publication in a more elaborate form, for it is hoped that these lectures may reach a public for which the large and expensive volumes of the *Introduction to the history of science* are rather forbidding.

There is good reason to believe that Dr. SARTON's Californian lectures will stimulate the "History of science" movement in this country. The University of California is already doing more work in that direction than any other American university, and indeed than any university, except London and Paris. That

work is still in a preparatory stage, but we may expect it to develop strongly as soon as economic conditions are sufficiently improved.

2. *Introduction to the History of Science.* Dr. WELBORN has continued her revision of the notes relative to Latin authors of the fourteenth century, chiefly physicians and mathematicians, and has amplified the bibliography considerably.

Dr. POGO prepared elaborate notes on various authors of the first half of the sixteenth century, chiefly of mathematicians and cosmographers of Central and Western Europe.

3. Other investigations. Dr. POGO has investigated the development of Egyptian water clocks from the time of AMENEMHET (XVIIIth dynasty, c. 1550 B.C.) to the Greco-Roman period. He has prepared a critical edition to the *Libellus de locorum describendorum ratione of* GEMMA FRISIUS (Antwerp 1533).

Dr. WELBORN is undertaking a study of medieval metrology, and has begun work on DINO DI GARBO's table of weights and measures. She has begun a transcription of the Canons of JOHANNES DE LINERIIS.

Dr. SARTON has prepared an account of the life and work of the great sixteenth-century mathematician, SIMON STEVIN of Bruges, and he has outlined the evolution of the decimal idea from the earliest times down to our own.

Miss VERA M. GUSHEE has been studying the development of scientific stellar astronomy before HERSHEL's time, with special reference to the works of THOMAS WRIGHT.

4. Publication of *Isis.* Dr. SARTON has edited with Dr. POGO's assistance, four numbers (52 to 55) forming the whole of vol. 18, and a half of vol. 19, a total of 934 p. with 10 pl. These four numbers contain 25 memoirs, 19 shorter papers, 31 reviews, 1721 bibliographical notes, and Mr. GUINET's index to vols. 16 to 18.

5. Lectures. In addition to the Hitchcock lectures already mentioned, Dr. SARTON delivered fourteen other lectures during his stay in California, most of them before various departments or seminars or groups of the University of California in Berkeley and in Los Angeles. He also spoke at Stanford University, and on Jan. 27 lectured in Pasadena under the auspices of the Mt. Wilson Observatory on The meaning of medieval science.

After his return to Cambridge he gave a course of about thirty-

five lectures on the history of science in the eighteenth and nineteenth centuries at Harvard University and a similar course at Radcliffe College.

Bibliography

Pogo, Alexander. See Sarton, George.

Sarton, George. Science and civilization in the Middle Ages with special reference to the Arabic writings. *Al-Kulliyyah*, vol. 18, 270-74, 366-73, Beirût 1932 (in Arabic).

— Remarks on the study and the teaching of Arabic. Macdonald presentation volume, 331-47. Princeton University Press, 1933.

— Preface to the nineteenth volume of *Isis* (Fez). *Isis*, 19, 6-10, 1933.

— , and Pogo, Alexander. Thirty-third Critical Bibliography of the history and philosophy of science, and of the history of civilization (to December 1931). *Isis*, 18, 334-429, 1932.

—, — Thirty-fourth Critical Bibliography of the history and philosophy of science, and of the history of civilization (to March 1932). *Isis*, 18, 440-540, 1933.

—, — Thirty-fifth Critical Bibliography of the history and philosophy of science and of the history of civilization (to August 1932). *Isis*, 19, 253-320, 1933.

The artifical fertilization of date-palms in the time of Ashur-nasir-pal B.C. 885-860.

The purpose of this note is to draw attention to facts which have long been known to Assyriologists but have not yet been integrated in the history of botany.

It is one of the curiosities of the history of science that the scientific knowledge of plant sexuality was not established until 1694 while an empirical knowledge of it in the cases of date-palm and fig-tree was immemorial (1). This contrast is the more remarkable because that empirical knowledge had not remained purely traditional but had been expressed in telling words by the early Greek writers.

In his history of botany JULIUS VON SACHS traced the literary accounts of plant sexuality back to THEOPHRASTUS, but he might have adduced a text a century earlier, namely the one found in the *History* of HERODOTUS (Book I, chapter 193), dealing with the fruitfulness of Babylonia :

> " Palm-trees grow in great numbers over the whole of the flat country, mostly of the kind which bears fruit, and this fruit supplies them with bread, wine, and honey. They are cultivated like the fig-tree in all respects, among others in this. The natives tie the fruit of the male-palms, as they are called by the Greeks, to the branches of the date-bearing palm, to let the gall-fly enter the dates and ripen them, and to prevent the fruit from falling off. The male-palms, like the wild fig-trees, have usually the gall-fly in their fruit " (2).

HERODOTUS was not a botanist and he had confused some of the facts concerning the date-palm with others concerning

---

(1) See my remarks in the *Introduction to the History of Science* (vol. 2, 62).

(2) GEORGE RAWLINSON : *History of* HERODOTUS. New edition (vol. 1, 267, 1862). I do not quote the Greek text as it is so easily available to those interested.

the fig-tree; this proves that he had heard of both. His mistake was corrected a century later by the botanist THEOPHRASTUS who speaks of the caprification (ἐρινασμός) of the fig, then proceeds:

"With dates it is helpful to bring the male to the female; for it is the male which causes the fruit to persist and ripen, and this process some call, by analogy, 'the use of the wild fruit (3).' The process is thus performed: when the male palm is in flower, they at once cut off the spathe on which the flower is, just as it is, and shake the bloom with the flower and the dust over the fruit of the female, and, if this is done to it, it retains the fruit and does not shed it. In the case both of the fig and of the date it appears that the 'male' renders aid to the 'female' —for the fruit-bearing tree is called 'female'—but while in the latter case there is a union of the two sexes, in the former the result is brought about somewhat differently" (4).

Some four centuries later similar ideas, were transmitted to the Latin world by PLINY. He explained caprification in chapter 21 of book XV of his Natural History, and dealt with the reproduction of the date-palm in chapter 7 of Book XIII. His account is on a much lower scientific level than that of THEOPHRASTUS but the sexual idea is emphasized as follows:

"It is asserted that in a forest of natural growth the female trees will become barren if they are deprived of the males, and that many female trees may be seen surrounding a single male with downcast heads and a foliage that seems to be bowing caressingly towards it; while the male tree, on the other hand, with leaves all bristling and erect, by its exhalations, and even the very sight of it and the dust from off it, fecundates the others: if the male tree, too, should happen to be cut down, the female trees, thus reduced to a state of widowhood, will at once become barren and unproductive. So well, indeed, is this sexual union between them understood, that it has been imagined even that fecundation may be ensured through the agency of man, by means of the blossoms and the down gathered from off the male trees, and, indeed, sometimes by only sprinkling the dust from off them on the female trees" (5).

* * *

The artificial fertilization described by THEOPHRASTUS was (or

(3) ὀλυνθάζειν, same meaning as ἐρινάζειν; ὄλυνθος and ἐρινός are names of wild figs used for caprification.

(4) THEOPHRASTUS: *Enquiry into plants*, with an English translation by Sir ARTHUR HOLT (2 vols., London, 1916, vol. 1, p. 155).

(5) English translation by JOHN BOSTOCK and H. T. RILEY (Bohn's Library, vol. 3, 172, 1855). Here is the end of the Latin text to illustrate its greater brevity and strength. "Adeoque est Veneris intellectus ut coitus etiam excogitatus sit ab homine e maribus flore ac lanugine, interim vero tantum pulvere insperso feminis."

seems to have been) illustrated as early as the first third of the ninth century, in the admirable bas-reliefs which adorned the royal palace of ASHUR-NASIR-PAL, King of Assyria from 885 to 860. These bas-reliefs were discovered by Sir AUSTEN HENRY LAYARD (1817-1894) at Nimrūd in 1845 (6) and are one of the chief glories of the British Museum. A series of beautiful reproductions of them were edited for that Museum in 1914 by (Sir) E. A. WALLIS BUDGE (7).

Thanks to the gracious permission of the Trustees of the British Museum I am able to reproduce here two of the plates bearing on the subject, among many others (8).

A few of the Nimrūd bas-reliefs can also be seen in the Metropolitan and in the Louvre. For a description of the latter consult E. POTTIER : *Catalogue des antiquités assyriennes* (Paris 1924), wherein references to earlier publications will be easily found. At least five of these bas-reliefs bear on our subject; see figs. 4, 5, 18, 21, 23. I reproduce on my plate III the most striking of them not from the photograph in POTTIER's book (fig. 5) but from a drawing made by SAINT ELME GAUTIER for PERROT and CHIPIEZ : *Histoire de l'art dans l'antiquité* (vol. 2, p. 64, 1884). This Louvre bas-relief is very similar to the British Museum one reproduced on pl. XXXVIII 2 of BUDGE's work, but it is even more significant for in this case the eagle-headed genius introduces the object which he is holding in his hand *into* the flower : his intention of touching the flower and reaching the very heart of it cannot be mistaken.

***

The great English anthropologist EDWARD BURNETT TYLOR (1832-1917) was the first to suggest (in 1890) that these scenes were symbolical representations of the artificial fecundation of the date-palm. Unfortunately the cryptic title of his paper " *The*

(6) The mound of Nimrūd on the east bank of the Tigris, about 20 miles to the south of Nineveh, marks the site of the biblical Calah (*Genesis*, X, 11).

(7) *Assyrian sculptures in the British Museum. Reign of* ASHUR-NASIR-PAL. 24 p., 53 plates small folio (31 × 26 cm.). London, British Museum, 1914.

(8) The following plates should be considered : XI, XXX, XXXII, XXXIII, XXXVIII, XXXIX, XL, XLII 2, XLIII, XLIV, XLV, XLVI, XLVII, XLVIII, L, LI, LII. Our plates I and II are reproductions respectively of XI and XLIV with the original legends of BUDGE's publication.

winged figures of the Assyrian and other ancient monuments " (9) caused it to remain unknown to the very people who would have taken the keenest interest in them, namely the historians of botany.

From HERODOTUS' and THEOPHRASTUS' testimonies, we may safely conclude that the artificial fertilization of dates and figs was known in Egypt and Mesopotamia in very early times. The cultivation of these plants and especially of the date-palm was of immense importance (10) and hence some incidents connected with it might easily assume a sacred character. TYLOR suggested that the sacred trees represented in these bas-reliefs were date-palms and that the conical objects brought close to the female flowers (or pointing in their direction) were the male inflorescences. His argument was considerably strengthened by the comparison illustrated on our plate IV. Fig. *b* is a detail from one of the Assyrian sculptures showing very distinctly the object which the genius is holding in his hand; fig. *d* is a drawing taken from KAEMPFER (11) of the male inflorescence of the date-palm stripped of its spathe and with the flowers open and ripe for scattering the pollen; figs. *a* and *c* are inflorescences photographed for TYLOR's paper. A comparison of figs. *a* and *b*—the former made from nature and the latter from an Assyrian monument—is especially instructive.

TYLOR's paper was soon discussed by EMMANUEL BONAVIA, W. ST. CHAD BOSCAWEN, TERRIEN DE LACOUPERIE, and Count GOBLET D'ALVIELLA (12). Much of that discussion is irrelevant

---

(9) *Proceedings of the Society of biblical archaeology* (vol. 12, 383-93, 4 pls. London, 1890).

(10) Cfr. HERODOTUS' statement quoted above.

(11) ENGELBERT KAEMPFER (1651-1716) : *Amoenitates exoticae* (Lemgo, 1712).

(12) EMMANUEL BONAVIA : Did the Assyrians know the sexes of date-palms? No (*The Babylonian and Oriental Record*, vol. 4, 64-69, 89-95, 116-17, 1890). The " No " does not appear in the title of the third instalment, and the author's conclusions are less categoric than the two earlier parts would have led one to expect.

W. ST. CHAD BOSCAWEN : Notes on the Assyrian sacred trees (*Ibidem*, vol. 4, 95-96, 1890).

TERRIEN DE LACOUPERIE : Stray notes on date-palms in Anterior Asia (*Ibidem*, vol. 4, 117-18, 1890); The calendar plant of China, the cosmic tree and the date-palm of Babylonia (*Ibidem*, vol. 4, 217-31, 246-51, 1890).

EUGÈNE GOBLET D'ALVIELLA : La fécondation artificielle du palmier dans la symbolique assyrienne (*Bulletins de l'Académie royale de Belgique*, vol. 20, 359-74, 1890); *La migration des symboles* (Paris, 1891).

EMMANUEL BONAVIA : *The flora of the Assyrian monuments and its outcomes*. (XXVI+215 p., 98 fig., Westminster 1894).

to our purpose, but the following points may be underlined. GOBLET D'ALVIELLA approved of TYLOR's thesis and amplified it. According to him the Assyrians being thoroughly familiar with the artificial fertilization of the date-palm and conscious of its importance raised its ceremonial practice to the dignity of a general symbol of fertility. This theory is generally accepted in England and is summarized in BUDGE's preface to the British Museum publication of 1914 above mentioned.

On the other hand BONAVIA held that the cone represented on the Assyrian monuments was not a date-palm inflorescence, but a cedar-cone used as an aspergillum, the bucket being then meant to contain holy water. This view—the pine or cedar cone view—does not interfere with the " sacred tree " idea (except that the tree may now be a conifer as well as a palm), but is inconsistent with the " sexual " idea. It is strongly defended by LÉON HEUZEY in the catalogue of the Assyrian antiquities kept in the Louvre (1924, p. 50-54, 55-56).

***

I had often looked at the Assyrian bas-reliefs of the British Museum and pondered over them without formulating any conclusion in my own mind, until TYLOR's paper came to my knowledge. TYLOR's comparison impressed me deeply and I am now inclined to think that his theory is true. Consider the following facts :

(1) Immemorial knowledge of artificial fertilization.

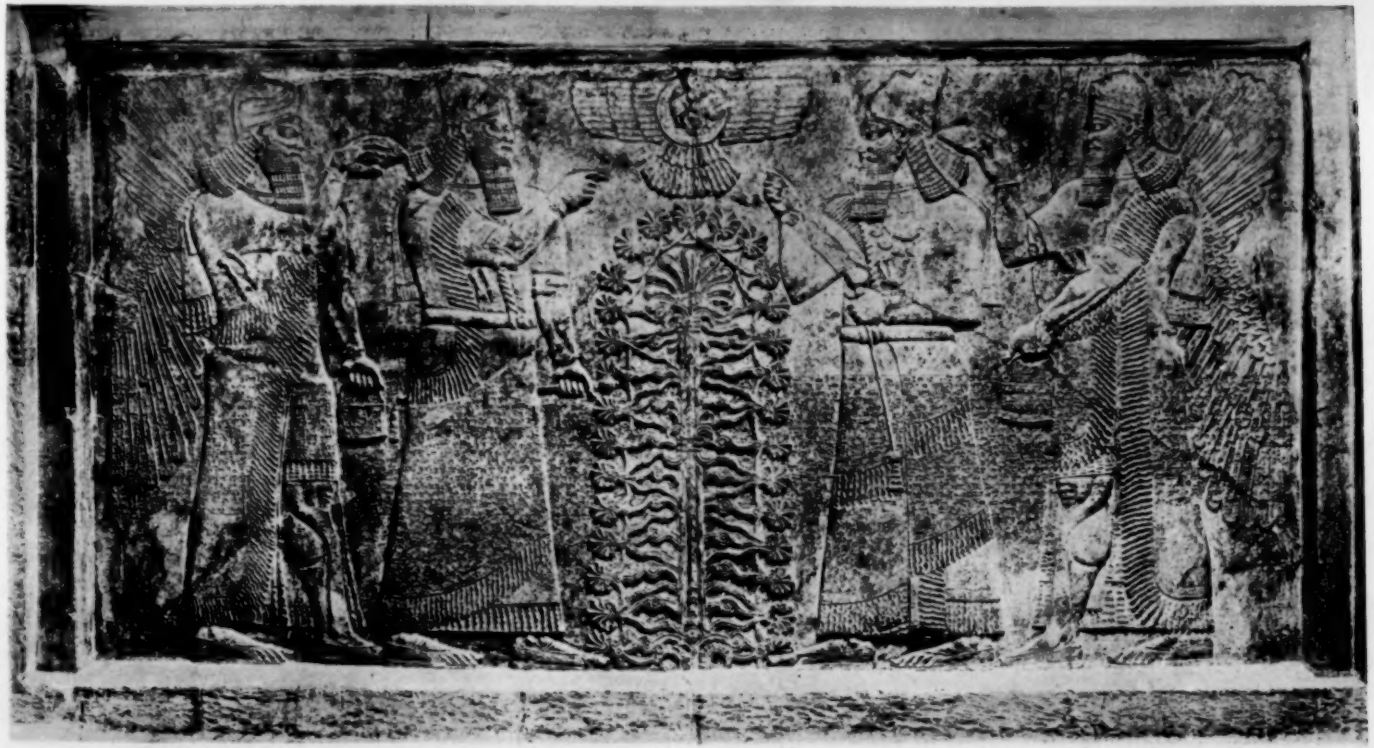
(2) Immense importance of the date-palm cultivation.

(3) Striking likeness between the Assyrian cones and the male date-palm inflorescences.

(4) Abundance of the Assyrian sculptures representing that " fertility " rite. (Could another rite less immediately significant have obtained the same popularity?).

If you put these facts together, does not the TYLOR conclusion seem more plausible?

Of course certainty is excluded. To obtain it, the monuments should be substantiated by Assyrian texts and I am not aware of any one referring directly or indirectly to the fructification of date-palms. HEUZEY adduces a Chaldaean magical formula.



Isis, XXI, pl. 1 G. SARTON.

Two figures of ASHUR-NASIR-PAL, assisted by winged beings, performing religious rites in connection with the ceremony of fertilizing a date-palm. The small figure within a winged circle above the "sacred tree" represents the god Ashur. Height 6 ft., 4 in. Width 14 ft., 2 in. (*Courtesy of British Museum;* BUDGE, 1914, pl. XI).

Isis, XXI, pl. 2 G. SARTON.

Eagle-headed, winged beings, in the character of fertilizers of the palm-tree, standing one on each side of the "sacred tree." Height 3 ft., 8 in. Width 5 ft., 4 in. (*Courtesy of British Museum;* BUDGE, 1914, pl. XLIV).

Isis, XXI, pl. 3 G. SARTON.

Louvre Museum. Height 103 cm. Width 88 cm. (After PERROT et CHIPIEZ, vol. 2, p. 64, 1884).

a

b

c

d

Isis, XXI, pl. 4 G. SARTON.

Fig. *a* and *c*. Male date-palm inflorescences from nature. Fig. *b*. Detail of Assyrian bas-relief. Fig. *d*. Male date-palm inflorescence, after KAEMPFER. These four figs. are taken from EDWARD B. TYLOR's paper of 1890.

" Prends le fruit du cèdre et présente-le à la face du malade " (13), and concludes triumphantly that this dispels every doubt " Après cela il n'y a plus qu'à donner, une fois pour toutes, le nom de pomme de cèdre à l'objet placé dans la main de notre figure et des autres semblables." However this does not convince me, for the generalization seems unwarranted.

The Nimrūd palace included a statue of the King ASHUR-NASIR-PAL, bearing a long inscription, but nothing in the latter is relevant to our purpose (14).

***

This paper may help historians of botany to realize the great antiquity of artificial fertilization of plants, which is sufficiently established by the early Greek texts referring to native Babylonian traditions. I hope that Assyriologists may find additional dated monuments bearing on this problem, and, what is even more important, texts explaining them, or referring otherwise to the cultivation of dates and figs. So many discoveries are being made in the field of oriental archaeology almost every day that our hope is not unreasonable.

To conclude, I should like to forestall questions and to try to explain the reasons for the extraordinary delay mentioned at the beginning of this paper.

The discovery of the sexuality of higher plants was kept in abeyance for at least two thousand years, because of the Aristotelian denial of sexuality in plants on a priori grounds, this prejudice obtaining more and more strength in the Middle Ages as the Master's influence increased. It was also delayed by the almost complete absence of experimental spirit in ancient times, and the excessive slowness of its growth down to the seventeenth century. This would be almost unconceivable to the scientifically trained man of to-day but for the experimental fact that he will still meet distinguished persons who are unable to consider any question except in an abstract and scholastic manner, without reference to simple and obvious experiments.

GEORGE SARTON.

(13) Quoted by HEUZEY (p. 52) after FRANÇOIS LENORMANT : *Origines de l'histoire d'après la Bible et les traditions des peuples orientaux* (vol. 1, Paris, 1880, p. 83-84).

(14) The statue is in the British Museum. Full translation of the inscription in BUDGE (1914, 23-24).

Die Alchemie des Avicenna

Nächst GEBER und RASIS wird in der lateinischen Alchemie kein Name öfters genannt als der des grossen Arztes und Philosophen AVICENNA. Keiner aber ist auch so umstritten, und so wird man es, nachdem die Diskussion seit BERTHELOT 40 Jahre geruht hat, gerechtfertigt finden, wenn ich hier den Versuch einer neuen kritischen Prüfung der AVICENNA zugeschriebenen Schriften vorlege.

Die Ansichten über die Echtheit der alchemistischen Schriften AVICENNA's schwanken bei den älteren Chemiehistorikern in weiten Grenzen. K. CHR. SCHMIEDER verhält sich vollständig ablehnend, wenn er in seiner *Geschichte der Alchemie* S. 97 das folgende Urteil fällt: „Zwar fehlt es garnicht an Schriften, welche man ihm zugeschrieben; nur hat man Ursache, sie allesammt für untergeschoben zu halten. Nirgend kommen arabische Originale vor, und die vorgeblichen Übersetzungen sind nicht beglaubigt. Sie sind erst seit 1500 lateinisch ausgegeben und abgedruckt worden, und wir verdanken sie wahrscheinlich unbekannten Wolthätern, welche uneigennützig uns den Verlust ersetzen wollten. Es ist demnach ein Pseudo-Avicenna, dessen Schriften nachfolgen...". SCHMIEDER konnte aber unmöglich wissen, ob es irgendwo noch arabische Originale gibt, und er konnte keine Beglaubigung für die Übersetzungen verlangen, wenn er überall sonst darauf verzichtete. Wie weit er sachlich dennoch das Richtige traf, werden wir später sehen.

H. KOPP beschränkt sich in seinen *Beiträgen* III, S. 55-59 auf einen Bericht über das Buch *De Anima*, betont seine Bedeutung als Quelle für VINCENTIUS BELLOVACENSIS und ROGER BACON, stellt auch fest, dass es „ungemein weit" hinter der *Summa* des GEBER zurückstehe, enthält sich aber jeder entscheidenden Stellungnahme.

M. BERTHELOT kommt in *La Chimie au Moyen Âge*, Bd. I,

S. 293 zu dem Urteil : „Quoique les textes arabes correspondants n'aient pas été signalés jusqu'ici, je ne vois, après étude des traductions latines, aucune raison valable pour contester ni l'existence des textes, ni l'attribution de ces textes à Avicenne".

Einen ähnlichen Standpunkt vertritt M. STEINSCHNEIDER, wenn er in seiner bekannten Abhandlung *Die Europäischen Übersetzungen* usw. B. (Wien 1905), S. 12 die Unterscheidung von Echtem und Unechtem für unerheblich erklärt und das Buch *De Anima* als ein „bis auf Weniges echtes Werk AVICENNAS" bezeichnet.

Es kann kein Zweifel bestehen, dass der von BERTHELOT und STEINSCHNEIDER vertretene Standpunkt wissenschaftlich unhaltbar ist. Es ist wirklich nicht unerheblich, weder für das Ansehen AVICENNAS, noch für die Geschichte der Alchemie, ob wir ihn für den Verfasser gewisser Schriften halten, oder ob diese Schriften von anderen Verfassern und aus anderen Zeiten herrühren. Es ist Pflicht des Historikers, wenn die äussere Beglaubigung versagt, aus der inneren Beschaffenheit der Texte, aus ihrem sachlichen und theoretischen Inhalt Kriterien der Echtheit oder Unechtheit zu ermitteln, und er darf erst dann seine Bemühungen einstellen, wenn sich zeigt, dass auch dieser Weg zu keinen sicheren Ergebnissen führt. Ohne solche kritische Arbeit wird die Geschichte der Alchemie, die es überwiegend mit pseudonymen und anonymen Texten zu tun hat, niemals aus ihrem Dämmerzustand herauskommen, ja man wird an den Aufbau einer wirklichen Geschichte überhaupt erst herantreten können, wenn diese kritische Prüfung an allen wichtigeren Texten durchgeführt worden ist.

Ob AVICENNA Schriften über Alchemie verfasst hat, lässt sich nur aus den ihm zeitlich nahestehenden biographischen und bibliographischen Quellen mit einiger Sicherheit beantworten, nicht aus späteren Nachrichten, die schon unter dem Einfluss von nachträglichen Fälschungen stehen können. Vollkommen einwandfrei sind im Grunde nur diejenigen Zeugnisse, die sich aus dem Studium der unzweifelhaft echten Werke des AVICENNA beibringen lassen.

Ich beginne mit den biographischen Nachrichten, die uns von AVICENNAS Schüler und langjährigem Begleiter, dem Astronomen ABŪ 'UBAID AL-JŪZJĀNĪ (1) teils nach Angaben des Meisters,

(1) Vgl. H. SUTER, *Die Mathematiker und Astronomen der Araber*. S. 172, unter Nr. 425 an zweiter Stelle.

teils nach eigenem Erleben überliefert wurden. Sie sind fast wörtlich übereinstimmend in der Gelehrtengeschichte des IBN AL-QIFṬĪ (2) und in der Ärztegeschichte des IBN ABĪ UṢAIBI'A (3) enthalten; eine kürzlich von P. KRAUS (4) veröffentlichte Übersetzung enthebt mich der Wiedergabe von Einzelheiten, wenn ich auch auf eine Skizze des Lebensganges nicht ganz verzichten möchte.

ABŪ 'ALĪ AL-ḤUSAIN IBN 'ABDALLAH IBN SĪNĀ, später allgemein als der „fürstliche Meister", *ash-shaikh ar-ra'īs* bezeichnet, ist zu Afshāna in der Nähe von Bokhara geboren. Sein Vater stammte aus Balkh, war ein Anhänger der Ismā'īlijja und bekleidete damals unter der Regierung des Samaniden NŪḤ IBN MANṢŪR (976-997) einen Verwaltungsposten. Frühreif und von unersättlichem Wissensdurst getrieben, war schon der Knabe mit mathematischen, astronomischen und philosophischen Studien beschäftigt. Mit 16 Jahren widmete er sich dem Studium der Medizin und der Jurisprudenz, mit 18 Jahren war er „mit allen diesen Wissenschaften zu Ende". Nach einem unsteten Wanderleben von einem Fürstenhof zum anderen, während dessen er schon zahlreiche philosophische, astronomische und medizinische Werke verfasste, blieb er längere Zeit in den Diensten 'ALĀ AD-DAULAS, des Herrn von Ispahan (reg. 1007-1041). Sein Freund, der Verfasser der Biographie, bat ihn damals, einen Kommentar zu ARISTOTELES zu schreiben, AVICENNA schlug ihm aber die Bitte ab und begann mit einem enzyklopädischen Werk über Logik, Naturwissenschaften, Mathematik und Theologie, das er *k. ash-shifā'*, d.i. „Buch der Heilung" nannte. Es ist sein philosophisches Hauptwerk geworden, dem er neben der Ausarbeitung des *Kanons der Medizin* und astronomischen Werken seinen Ruhm verdankt. Auch philologischen Arbeiten, zu deren Abfassung er in den letzten Jahren seines Lebens veranlasst wurde, spendet AL-JŪZJĀNĪ höchstes Lob. Eine Gewaltnatur, masslos im Genuss wie in der Arbeit, erlag AVICENNA mit 53 Jahren in Hamadhan wiederholten Kolikanfällen. Dort wird auch sein Grab gezeigt,

(2) J. LIPPERT, *Ibn al-Qiftīs ta'rīkh alḥukamā'*, S. 413.

(3) Ed. A. MÜLLER, Bd. II, S. 2-9. Vgl. auch G. SARTON, *Introduction*, Bd. I, S. 709 ff. und CARRA DE VAUX, *Avicenna*, S. 131 ff.

(4) P. KRAUS, *Eine arabische Biographie Avicennas*. Klinische Wochenschrift, Bd. 11, 1932, S. 1880-1884.

doch soll er später nach Ispahan überführt und dort beigesetzt worden sein.

So wenig in AL-JŪZJĀNĪS biographischen Aufzeichnungen ein Hinweis darauf zu finden ist, dass sich AVICENNA mit Alchemie befasste, so wenig geben die Listen seiner Werke darüber Aufschluss : und doch ist es undenkbar, dass ein Arzt und Naturforscher von einer der bestentwickelten Wissenschaften seiner Zeit keine Kenntnis gehabt hätte. BERTHELOT bemerkt ganz richtig, dass AVICENNA, auch wenn er nicht an die Transmutation glaubte, durchaus nicht davon abzusehen brauchte, die beobachteten Tatsachen und Verfahren zu beschreiben. Aber ebenso richtig ist, dass er bei skeptischer Einstellung zur Alchemie keinen Anlass hatte, ihr durch seinen Namen Vorspanndienste zu leisten. Mit all solchen Erwägungen von Möglichkeiten kommen wir keinen Schritt weiter, und wir müssen uns schon in den Schriften selbst umsehen, um zu festen Ergebnissen zu gelangen.

Wir erhalten den gewünschten Aufschluss aus zwei Schriften : aus einem Kapitel der bereits erwähnten Enzyklopädie, von dem nachher ausführlicher die Rede sein soll, und einer kleineren Abhandlung, die sich mit der Widerlegung der Astrologen befasst und den Titel *iṣḥārat ilā 'ilm fasād aḥkām al-nujūm* „Aufzeigung der Wertlosigkeit der Astrologie" führt (5). Hier nennt AVICENNA unter den Pseudo-Wissenschaften, mit denen sich kein Gelehrter beschäftigen dürfe, der auf seinen Ruf halte, auch die Alchemie : ihre Wurzel sei die Habsucht und die Neigung zu bequemem Leben, ihr Inhalt die Gewinnung eines Mittels, von dem man behaupte, dass es jedes gemeine Metall ohne Mühe in Gold oder Silber verwandle. Es gebe viele Bücher darüber, aber was JĀBIR, RĀZĪ u.a. geschrieben hätten, seien nur Absurditäten. Was Gott durch natürliche Kräfte erschaffen habe, könne nicht künstlich nachgemacht werden, und umgekehrt hätten die Werke des Menschen nichts mit denen der Natur gemein (6).

Diese grundsätzliche Ablehnung der Alchemie lässt schon nichts zu wünschen übrig. Noch eingehender behandelt AVICENNA die Frage in den von den Mineralen handelnden Teilen des *Buchs*

(5) Vgl. A. F. MEHREN, *Vues d'Avicenne sur l'astrologie*, etc., Le Muséon, Louvain 1885, S. 5.
(6) Ebenda, S. 7.

der Heilung. Die zugehörigen Kapitel sind um 1200 von ALFRED VON SARESHEL übersetzt worden und als *Liber de Congelatione et Conglutinatione Lapidum* bald unter dem Namen des AVICENNA, bald unter dem des GEBER, daneben auch als *Liber Aristotelis de Mineralibus* gedruckt erschienen. Für alle bibliographischen Einzelheiten kann ich auf die schöne Ausgabe von HOLMYARD und MANDEVILLE verweisen, der ich auch die Übersetzung der Stellen entnehme, die für unsere Untersuchung in Betracht kommen (7).

AVICENNA unterscheidet in dem Kapitel über die Bildung der Minerale vier Gruppen, die im wesentlichen denen im *k. sirr alasrār* des RĀZĪ entsprechen : die Steine, die Metalle, die Schwefel und die Salze. Das Quecksilber gehört zu den Metallen, da es die wesentliche Grundlage aller Metalle ist. Auf die verwickelten Theorien, die er an einer späteren Stelle über die Natur des Quecksilbers vorträgt, kann ich hier nicht eingehen; in der Lehre von der Entstehung der Metalle folgt er der bekannten Quecksilber-Schwefel-Theorie. Den Anspruch der Alchemie, Gold oder Silber herstellen zu können, weist er mit folgenden Worten (8) zurück :

There is little doubt that, by alchemy, the adepts can contrive solidifications in which the qualities of the solidifications of mercury by the sulphurs are perceptible to the senses, though the alchemical qualities are not identical in principle or in perfection with the natural ones, but merely bear a resemblance and relationship to them. Hence the belief arises that their natural formation takes place in this way or in some similar way, though alchemy falls short of nature in this respect and, in spite of great effort, cannot overtake her.

As to the claims of the alchemists, it must be clearly understood that it is not in their power to bring about any true change of species. They can, however, produce excellent imitations, dyeing the red (metal) white so that it closely resembles silver, or dyeing it yellow so that it closely resembles gold. They can, too, dye the white (metal) with any colour they desire, until it bears a close resemblance to gold or copper; and they can free the leads

(7) *Avicennae de congelatione et conglutinatione lapidum, being sections of the k. al-shifā'* usw., Paris 1927. Vgl. auch I. HAMMER-JENSEN, *Das sog. IV. Buch der Meteorologie des Aristoteles*, Hermes, Bd. 50, 1915, S. 113-136.

(8) Nach der Übersetzung von HOLMYARD, a. a. O., S. 40.

from most of their defects and impurities. Yet in these (dyed metals) the essential nature remains unchanged; they are merely so dominated by induced qualities that errors may be made concerning them, just as it happens that men are deceived by salt, qalqand, sal-ammoniac etc.

I do not deny that such a degree of accuracy may be reached as to deceive even the shrewdest, but the possibility of eliminating or imparting the specific difference has never been clear to me. On the contrary, *I regard it as impossible*, since there is no way of splitting up one combination into another. Those properties which are perceived by the senses are probably not the differences which separate the metals into species, but rather accidents or consequences, the specific differences being unknown. And if a thing is unknown, how is it possible for anyone to endeavour to produce it or to destroy it?

Man sieht, dass AVICENNA den Alchemisten die Möglichkeit von Färbungen, d.h. die Herstellung von Fälschungen zugesteht, die selbst den Erfahrensten täuschen können, dass er aber die Überführung einer Metallart in eine andere mit fast modern klingender Begründung für unmöglich erklärt. Welche Gründe hätte er nun haben können, später von dieser Anschauung abzuweichen, da doch die Misserfolge der Alchemie auf der Hand lagen? Jedenfalls wissen die besten späteren Gewährsmänner, AL-JILDAKĪ (9) und IBN KHALDŪN (10), nichts von einem Wandel seiner Überzeugung, und so haben wir nur noch zu untersuchen, welches Gewicht den Nachrichten von seiner Bekehrung zur Alchemie beizulegen ist, und welche Bewandtnis es mit den alchemistischen Schriften hat, die er verfasst haben soll.

An einer von E. WIEDEMANN beigezogenen Stelle einer Hs. des India Office (Kat. LOTH 1043, fol. 208 *a*) wird gesagt, dass AVICENNA, nachdem er im *Buch der Heilung* die Alchemie zu widerlegen unternommen hätte, später eine Abhandlung *ḥaqā'iq al-ishtihār* (Wahrheiten des Bekanntgebens) zu ihrer Verteidigung geschrieben habe (11). Aber eine solche Bemerkung musste in

(9) HOLMYARD und MANDEVILLE, a. a. O., S. 7.

(10) HOLMYARD und MANDEVILLE, a. a. O., S. 5.

(11) E. WIEDEMANN, *Zur Alchemie bei den Arabern*. Abh. z. Gesch. d. Naturw. u. d. Medizin, Heft 5, Erlangen 1922, S. 26.

der älteren Zeit jeder Fälscher voranschicken, der sich noch des Widerspruchs seiner Absichten mit der tatsächlichen Einstellung AVICENNAS bewusst war. Die Schrift scheint verloren zu sein; sollte sie noch irgendwo auftauchen, so zweifle ich nicht, dass ihre Unechtheit nachgewiesen werden kann. Auch über die von AL-JILDAKĪ erwähnte und von ḤĀJJĪ KHALĪFA Bd. V, S. 279 unter Nr. 11734 angeführte Abhandlung *mir'at 'ajā'ib fi'lkīmiyā* „Spiegel der Wunder in der Chemie", lässt sich nichts bestimmtes sagen. Bei einer anderen von E. WIEDEMANN nach der Ärztebiographie des IBN ABĪ UṢAIBI'A Bd. II, S. 19 erwähnten Schrift, dem „Sendschreiben an den Scheich ABU'L-ḤASAN SAHL IBN MUḤAMMAD AS-SAHLĪ über die Alchemie" muss ich aber länger verweilen, da sie offenbar mit der berühmten *Epistola ad Regem Hasen de re recta* identisch ist (12) und das arabische Original, wie P. KRAUS bei der Durchsicht des indischen Handschriftenkatalogs *Tadhkirat alNawādir* (13), entdeckte, in zwei indischen Bibliotheken noch existiert. Ich beziehe mich im folgenden auf den im *Theatrum Chemicum*, Bd. IV, 1659, S. 863 ff. abgedruckten Text, da mir im Augenblick kein besserer zu Gebote steht.

Man sieht bald, dass die Übersetzung so ungeschickt und unklar ist, dass zu ihrem vollen Verständnis die Vergleichung mit dem arabischen Original notwendig wäre. Das unverständliche *de re recta* kann ich schon aus den Angaben der *Tadhkira* richtigstellen: der arabische Text lautet *fi'lamr al-kulliyy*, d.h. *de re universali* oder *universa*. Die ganze Schrift besteht aus 10 Kapiteln, die in dem Druck des *Th. Chem.* 12 Druckseiten füllen. AVICENNA erzählt, dass er mit HASAN viele Aussprachen über die Alchemie gehabt habe, weshalb dieser ihn gebeten habe, seine Gedanken zu Papier zu bringen. Er habe die Bücher der Anhänger wie die der Gegner der Alchemie studiert, aber ihre Gründe und Gegengründe schwach gefunden. Darum habe er seine Zuflucht zum eigenen Nachdenken genommen und untersucht, ob es ein Elixir gebe, und wie es beschaffen sei (14). Dann beginnt eine

(12) Über Handschriften vgl. D. W. SINGER, *Catalogue*, S. 116, N° 126 und Carini, Riv. Sicula, Bd. VII, 1872, S. 54, N° 18 und S. 59, N° 21. Einen von mir nicht näher untersuchten Text enthält auch die Hs. Göttingen. Hist. nat. 75.

(13) HYDERABAD 1350 d. H. (1931), S. 178, N° 309.

(14) Text: 'Si hoc est, quomodo est, et quomodo non est.'

konfuse Aufzählung der für das Gelingen der Färbung notwendigen Bedingungen, die mit den Sätzen schliesst, dass keiner der Weisen eine der Medizinen gekannt habe, von denen AVICENNA berichtet. Es handelt sich dabei um die Gewinnung einer *substantia quae permisceatur liquefactis*, einer *s. solidativa*, *s. coagulativa*, *s. adunativa* und *s. fixa super ignem*, die alle zu einer einzigen Substanz verschmolzen werden sollen : *si ergo pervenimus ad has quinque res, pervenit ad nos intentio nostra.*

Im zweiten, umfangreichsten Kapitel wird über die weisse und rote Tinktur gehandelt. Für die weisse Tinktur muss man wissen, dass AVICENNA gefunden hat, wie das *zaimbar vivum* (15) geweisst werden kann, für die rote hat AVICENNA alle Dinge gefunden, die in das Silber eindringen. In Kapitel III *De substantia permiscibili liquefacto* verdient ein Zitat aus JĀBIR und der Satz *Sed nos quidem scivimus illud ante experimentum, cum ratiocinatione quam verificavit experimentum* Beachtung. Durch die Komposition aller Substanzen entsteht nach Kapitel VI *res una, stans, tingens, profundens, consolidans, perseverans.* Damit könnte man die Aufgabe der Herstellung des Elixirs für gelöst halten, aber wir hören, dass der Erfolg bisweilen ausbleibt, *aut propter debilitatem instrumentorum aut defectum in opere aut propter res particulares accidentales, quae non comprehenduntur*, und so gibt der Verfasser noch ein Kapitel über die Tierstoffe zum Besten, das mit den Worten beginnt : „Postquam audivimus multotiens quod possibile est *ex capillis, ovo et sanguine* et multis partium animalium, quod fiat *elixir*, incepimus ergo experiri, an haberent suspensionem et impressionem in corporibus liquefactis. Invenimus itaque ea non imprimere, sed fumum eorum... Et scivimus quod primum quod est ex eis distillatum, est *aqua*, deinde *oleum*, quoniam fixius est super ignem" usw. Diese Ausführungen, in denen auch der Salmiak viel genannt wird, enthalten im wesentlichen die aus dem *Buch der Siebzig* bekannte Lehre des JĀBIR IBN ḤAYYĀN. Ich zitiere noch die Schlussworte : „Et scias quando bene vaporat totum, tunc iam bene operatus es in compositione, sed errasti in calce. Infrigida ergo ipsum, et ingenies, ut addas in tinctura et suspensione, tamen non sit in hoc exitus a termino, ita quod totum evaporet".

(15) *Zaimbar* ist *Zaibac*, Quecksilber.

Ist dieser Traktat nun wirklich ein Brief des AVICENNA an den Scheich ḤASAN, oder ist er von einem Alchemisten verfasst, der seiner Lehrschrift durch den Namen des berühmten Arztes höhere Geltung verschaffen wollte? Entgleisungen und Anachronismen, die den Fälscher sicher verraten, habe ich in der kurzen Abhandlung nicht entdecken können; so muss sich der Nachweis auf innere Gründe zu stützen versuchen. Der Eingang ist nicht ungeschickt abgefasst, aber die Behauptung, dass AVICENNA nach Prüfung der Gründe von Anhängern und Gegnern, die er beide ungenügend fand, aus eigenem Nachdenken sofort entdeckt habe, *quod possibile est, ut Lunam in colorem Solis tingamus, et Venerem in colorem Lunae*, steht mit der Tatsache in Widerspruch, dass er sich in einer zweifellos echten Schrift aufs Schärfste gegen die Möglichkeit der Metallverwandlung ausgesprochen hat. Wer den Brief an HASAN geschrieben hat, weiss davon nichts, oder unterschlägt den wahren Sachverhalt. AVICENNA hätte seine prinzipielle Gegnerschaft einem Manne gegenüber, mit dem er angeblich zahlreiche Aussprachen gehabt hat, nicht ableugnen können.

Dies ist der eine schwache Punkt, an dem sich der Fälscher verrät. Aber auch der ganze auf die Gewinnung der „Medizin" bezügliche Inhalt löst Zweifel aus. Ein Naturforscher, der mit so klarem Blick für die Wirklichkeit über mineralogische Fragen geschrieben und die Anschauungen von JĀBIR und RĀZĪ als absurd bezeichnet hat, kann unmöglich später geglaubt haben, dass man aus destillierten Haaren, Eiern oder Blut ein Elixir gewinnen könne, das Silber in Gold verwandelt.

Ob die *Epistola ad Hasen* im Osten oder in Spanien verfasst worden ist, lässt sich aus dem Inhalt der Schrift zurzeit nicht feststellen. Der Umstand, dass noch eine Handschrift in Indien vorhanden ist, könnte auf östliche Herkunft schliessen lassen, aber sogut die Werke des GEBER, RASIS und AVICENNA nach Spanien gekommen sind, kann auch einmal eine spanische Schrift den Weg nach dem Osten gefunden haben. Dass das Werk schon VINCENTIUS BELLOVACENSIS in Übersetzung vorlag, wie die Anführungen im *Speculum Doctrinale* zeigen, spricht eher für spanischen Ursprung.

Um 1330 beschäftigt sich PETRUS BONUS in seiner *Margarita Novella Pretiosa* mit den Widersprüchen zwischen dem *Tractatus*

ad Asten de Retia (16) und der Schrift *De Congelatione*, die er als des ARISTOTELES *Liber quartus Meteororum* oder *Metaphysicorum* zitiert. Er weiss, dass andere die Schrift, oder wenigstens die in ihr enthaltene Ablehnung der Alchemie, dem AVICENNA zuschreiben, und widerlegt die Autorschaft des AVICENNA durch den Hinweis, dass dieser doch *in tractatu suo ad Asten de Retia* für die Wahrheit der Alchemie eingetreten sei (17).

Hat die *Epistola ad Regem Hasen* für die Geschichte der Alchemie nur untergeordnete Bedeutung, so muss dem *Liber Abvali Abincine de Anima in arte Alchimiae* als einer Quellenschrift ersten Ranges eine umso gründlichere Untersuchung gewidmet werden (18). BERTHELOT und STEINSCHNEIDER haben die Schrift als ein echtes, nur durch einige Interpolationen erweitertes Werk erklärt; ich werde nachweisen, dass es eine spanisch-arabische Fälschung ist, die etwa am Anfang des 12. Jahrhunderts verfasst wurde. Dass die lateinische Übersetzung von einem in Spanien lebenden Alchemisten herrührt, hat schon BERTHELOT gesehen; auch dafür werde ich weitere Beweise beibringen.

Wäre das Werk echt, so müsste es an einem der Höfe geschrieben sein, an denen AVICENNA sein Leben zubrachte; man müsste also erwarten, dass es nach dem Brauch der Zeit dem Fürsten gewidmet wurde, zu dessen Umgebung AVICENNA zur Zeit der Abfassung gehörte. Man findet aber in dem ganzen Werk keine Spur einer Anspielung auf die Verhältnisse des Ostens, und ebenso fehlen, abgesehen von ganz allgemeinen Ortsangaben bei einigen Mineralien, alle geographischen Anspielungen auf den Osten, während eine Reihe von westlichen, insbesondere spanischen

(16) Dies steht, wie die Prüfung der Anführungen zeigt, für *Hasen de re recta*. Der Titel des Traktats hat auch sonst schwere Entstellungen erfahren. So lautet er im Codex SPECIALE (vgl. Rivista Sicula, Bd. VII, 1872, S. 59) *Incipit Epistola principis albolay cognominis albisceni ad hakasen de re tecta;* daneben kommen auch die Varianten *hahassen*, *haasen* und *asen* vor.

(17) Vgl. Theatrum Chemicum, Bd. V (1660), S. 541. Im Druck steht nach 540 die falsche Seitenzahl 542.

(18) Die einzige Druckausgabe ist die in *Artis Chemicae Principes Avicenna atqve Geber*, Basel 1572, S. 1-471 enthaltene. Das Werk wird von SCHMIEDER unter dem Titel *Porta elementorum* erwähnt, der nur für die Einleitung gilt. STEINSCHNEIDER führt Einleitung und Hauptteil als besondere Werke an, obwohl schon KOPP den Sachverhalt kannte. Über die Pariser Hs. 6514 vgl. BERTHELOT, *La Chimie au Moyen Age*, Bd. I, S. 293 ff., über die englischen Hss. den *Catalogue* von D. W. SINGER unter Nr. 123.

Ortsnamen mit Bestimmtheit auf einen spanischen Mauren als Verfasser schliessen lassen. Zahlreiche unübersetzt gebliebene arabische Ausdrücke stellen das Vorhandensein eines arabischen Originals ebenso sicher, wie eingestreute spanische Worte den spanischen Übersetzer verraten. Ich belege das Gesagte durch eine Reihe von Beispielen.

Beweisend sind nicht die in allen alchemistischen Traktaten auftretenden Lehnworte *alembic*, *aludel*, *azoch*, *tutia* usw., sondern seltene Ausdrücke, die nur unmittelbar aus einer arabischen Vorlage stammen können, wie *bellote* i.e. glandes (S. 436) das arab. *ballūṭ* Eichel, *attozongi* und *artozongi* arab. *atrunj* Zitrone, *orrez* S. 418 *ar-rīz* Reis wiedergibt, oder *zoala* für *Zuḥal*, d.i. Saturn und *amostari* für *Mushtarī*, d.i. Jupiter steht. Auf Spanien weist ganz besonders das an vielen Stellen vorkommende Wort *morabunt* bezw. *morabetini*, der Name einer Goldmünze, die zur Zeit der Almoraviden *(al-Murābiṭūn)* geprägt wurde. Da ihre Herrschaft in Spanien im Jahre 1149 ein Ende nahm, ergibt sich daraus ein Anhaltspunkt für die Abfassungszeit des Buches.

Für die spanische Heimat des Übersetzers hat schon BERTHELOT das Wort *plata* angeführt, das gelegentlich statt *argentum* gesetzt wird; ich erwähne noch *alcarcadre*, *bacino*, *barchino*, *berdolagos*, *cascanellos*, *cazola*, *michilla*, *orella*, *raton*, *sosa*, *tartuca*, ohne in jedem Falle feststellen zu können, ob die Worte in ähnlicher Form auch in anderen romanischen Sprachen auftreten.

Von Stoffen mit westlichen Fundorten nenne ich Cuprum *Navarrae* S. 114, ferrum de *Landelum*, d.i. Andalusien S. 123, sal nitrum de *Andeluz* S. 135, sal nitrum de *Seduna* S. 135 und auripigmentum de *Xeduna* S. 143, d.i. (Medina) Sidonia, arab. *Shadhūna;* sulfur de *Burchina* S. 142 und auripigmentum de *Alburchen* de Sicilia S. 143, d.i. die Insel Volcano oder der Vulkan Siziliens, also der Aetna. Die Namen *Allodua*, *Albozani*, *Aldeua*, *Achudawa*, *Albuaria* und *Forperia* habe ich noch nicht identifizieren können. Plumbum de *Enchachia* S. 115 ist Blei aus Antiochia, wie sich aus dem daneben vorkommenden *anthachi* ergibt, aurum *xarchi* und *exarchi* S. 130 ist *ash-sharqī*, östliches Gold, baurach *espini* S. 383 ist Borax von Ispahan, b. *acerabam* B. von *Zerawand* (19).

(19) Die von G. WEIL in KOPPS *Beiträgen* III, S. 56 gegebenen Deutungen

Bevor ich mich dem Inhalt zuwende, sei noch ein Wort über den Titel und die Kapiteleinteilung des Werkes gesagt. Der Titel mag arabisch *kitāb al-nafs fi'l-kīmiyā* gelautet haben. Vielleicht ist er von dem Fälscher gewählt, weil AVICENNA selbst einige Schriften mit ähnlichem Titel verfasst hat (20). Wir finden aber auch eine ausführliche Begründung des Titels am Schluss des ganzen Werks, wo es S. 470 heisst :

„Dixit Abuali Abincine : istum librum feci in anima, et nominavi eum *librum de Anima :* Ideoque (lies : ideo quod) altior est corpore, et non potest videri oculis, sed mente : quia oculus tantum accidens videt, et mens videt proprietatem : et ideo altiora sunt ea quae videntur oculis. Et anima est quaedam pars circuli gloriae, et circulus animae est altior omnibus circulis de corpor(al)ibus circulis, et de spiritualibus..."

Auch der Zusammenhang zwischen der *Porta elementorum* und den nachfolgenden zehn Dictionen wird S. 23 ausdrücklich hergestellt : „Laudetur Deus, quia complevimus Portam elementorum : et incipiemus primam Dictionem de decem dictionibus libri nostri, in nomine Domini". Auch manche Kapitelüberschriften werden ausdrücklich begründet; ob aber alle Zwischentitel innerhalb der Kapitel schon im arabischen Original vorhanden waren, ist zu bezweifeln, da sie in vielen Fällen eine Form aufweisen, die nicht vom Verfasser herrühren kann (21).

Da schon VINCENTIUS BELLOVACENSIS die lateinische Übersetzung als *Liber alchimiae qui dicitur de Anima* ausgiebig benützt hat und auch ROGER BACON das Werk kennt, muss die Übersetzung schon in der ersten Hälfte des 13. Jahrhunderts vorhanden gewesen sein. Für die Abfassung des Originals kommt frühestens das

---

geographischer Namen sind bis auf *elherachi=al'irāqī* verfehlt. *Herminia* ist nicht ein Ort bei Buchara, sondern Armenien; danach ist auch baurach *ermini, armini, carmini* und *canimi* (S. 383) armenischer Borax. *Taliconi* ist nicht eine Provinz in Nordpersien, sondern das Wort tâliqūn, d.h. *μεταλλικόν*. *Accingar* ist nicht eine Stadt bei Mossul, sondern das Wort *azzinjār*, d.i. Grünspan. Auch bedeutet *calh* nicht gelblich, sondern ist die Umschrift von *qala'ī*, das ' Zinn ' bedeutet.

(20) Vgl. C. BROCKELMANN, *Geschichte der arabischen Literatur*, Bd. I, S. 455, Nr. 29-32.

(21) So etwa S. 57 : De filio qui non intelligebat superflua quae dixerat pater; S. 68 : Disputatio contra Abubachar, et de commendatione Rasis; S. 276 : Pater castigat filium; S. 298 : Istud capitulum non est de libro, sed est inter patrem et filium, et seniorem; S. 303 : De monte (lies : morte) Arami usw.

Ende des 11. Jahrhunderts in Frage, d.h. die Zeit zwischen der Abfassung der *Rutba* des Pseudo-MAJRĪṬĪ und der des AL-RĀZĪ zugeschriebenen Buches *De Aluminibus et Salibus*, aus dem VINCENTIUS ebenfalls geschöpft hat. Auf das Jahr genau wäre die Zeit der Übersetzung bestimmt, wenn eine merkwürdige Stelle gegen Ende der Dictio X als Angabe des Übersetzers gedeutet werden darf. Dort findet sich mitten in dem mit *Decima* (sc. *desponsatio*) bezeichneten Abschnitt S. 767 der Satz „*Completus est liber, deo gratias*", und nach einigen Ermahnungen an den Sohn, das Buch über die Gewichte zu einer Leuchte zu machen, der Satz : „*Finit. CCXXIII anni sunt transacti, ex quo Abincenus perfecit hoc opus. Et iam erant transacti M et CC et XXXV anni*". Ich ergänze hier *post Christum natum*, und nehme an, dass der Übersetzer sagen will, dass AVICENNA das Buch 223 Jahre vor der Zeit des Übersetzers, dem Jahre 1235, verfasst habe. Man käme damit zu der ganz annehmbaren Jahreszahl 1012, die der arabische Fälscher, in Jahren der Hidschra ausgedrückt, am Schluss seines Werkes angegeben haben kann.

Das Werk ist von einem Prologus und einem Nachwort umrahmt, in denen der Inhalt des Buches erläutert wird. Beide stimmen auf längere Strecken fast wörtlich überein. Uns interessiert hier besonders der Eingang des Prologs mit der Begründung des Titels, die in folgenden Worten gegeben wird : „Dixit Abuali Abincine : Explanabo tibi, fili mi, quot capitula continentur in una quaque dictione [*sive* libro] libri unius (lies : huius) qui dicitur *de Anima :* quia librum istum intellectione nominavi eum librum *de Anima*, ideo quia anima altior est corpore etc.".

Die *Tabula Portae Elementorum* und die *Tabula X Dictionum* mit den Aufzählungen der Kapitel scheint ebensowenig ursprünglich zu sein, wie die ausführlichen Inhaltsangaben, die jeder „Dictio" vorangesetzt sind. Der Umfang der Dictionen ist sehr ungleich; Dictio VI macht mit 33 Kapiteln auf 220 Druckseiten beinahe die Hälfte des ganzen Werkes aus, dann folgen der Grösse nach Dictio I mit 12 Kapiteln auf 63 Seiten, V mit 29 Kapiteln auf 48 Seiten, VII mit 5 Kapiteln auf 42 Seiten, VIII mit 2 Kapiteln auf 17 Seiten. Die übrigen Dictionen haben weniger als zehn Seiten Umfang.

Sieht man nur auf die grossen Linien, so lässt sich dem Gesamtwerk eine einheitliche Disposition nicht aberkennen. In Dictio I

wird erörtert, ob es eine Alchemie gibt, in II, was die Alchemie ist, in III, wie die Alchemie zustande kommt, in IV, warum es eine Alchemie gibt. Dann handelt V von den Namen und der Natur der Steine und anderer Dinge, die für das Magisterium notwendig sind, VI von den Operationen, die mit Körpern und Geistern durchgeführt werden müssen, bevor man zur Darstellung des Elixirs kommt; in VII beschäftigt sich der Verfasser mit dem aus Blut, Eiern oder Haaren darzustellenden Elixir, in VIII mit den Fermenten, in IX mit der Complectio magisterii et sponsalitii, in X mit den Gewichten. Von der Mannigfaltigkeit des Inhalts und dem starken Wechsel der Darstellungsform kann nur gründliches Studium eine wirkliche Vorstellung geben. Ich hoffe, dem Leser dadurch einen Einblick in die Eigenart des Werkes vermitteln zu können, dass ich verwandte Dinge zusammenfassend behandle.

Was das Verhältnis der Übersetzung zum Original anlangt, so habe ich den Eindruck gewonnen, dass wir es mit einer im grossen und ganzen intakten Überlieferung zu tun haben, wenn auch an gewissen Stellen offenbar Zusätze des Übersetzers vorliegen. Die Übersetzung scheint mit grosser Gewissenhaftigkeit, wenn auch nicht immer mit genügendem Verständnis gemacht zu sein. Sie bewahrt jedenfalls ziemlich treu den arabischen Satzbau und versagt nur öfters bei der Wiedergabe von Eigennamen (22). Die Doxologien, die sich immer wiederholende Wendung *In nomine Domini dixit Abuali Abincine* und viele andere Züge, auf die ich hier nicht eingehen kann, sind ebensoviele Kennzeichen treuer Überlieferung.

Die Ruhe des Lehrvortrags und die Eintönigkeit der technischen Vorschriften wird unterbrochen, wo die Darstellung in den Dialog übergeht oder wo sich zwischen Vater und Sohn dramatische Szenen abspielen. Sie sind schon BERTHELOT aufgefallen und er bezeichnet sie als „intermèdes humoristiques, où le disciple refuse de croire son maître et de lui obéir". Es kommen aber auch Szenen vor, die man fast nur als eine Verhöhnung der Alchemie empfinden kann, und die in der ganzen alchemistischen Literatur, soweit ich sie kenne, kein Seitenstück haben.

---

(22) Ein grosser Teil der Entstellungen mag unwissenden Abschreibern zur Last fallen.

Die erste Frage stellt der Sohn ABUZALEM an den Vater S. 65, am Ende der grossen Auseinandersetzung mit den Hauptvertretern der Alchemie : „Ego non intelligo praedicta superflua verba, sed dic mihi verbum, quod intelligere possum". Nach der *Disputatio contra Maurienum* fragt ABUZALEM, warum ABUALI jenen Meister verachte, und erhält zur Antwort, dass er wissen müsste, warum der Meister das eine lobe und das andere verachte, wenn er ihn verstanden hätte. In einem Kapitel über die Decknamen S. 76 sagt der Meister : „Verstehe : wenn ich schwöre, dir die Wahrheit zu sagen, musst du glauben, dass es eine Lüge ist, und umgekehrt; du darfst das aber niemand sagen, weil ich dir hier ein grosses Geheimnis verrate, das der unwissenden Menge verborgen bleiben muss". S. 85 werden vier grosse Lügen angegeben, die der Schüler nicht glauben soll, aber am Schluss heisst es doch wieder : „Benütze dieses Buch, so wirst du der Herr der ganzen Welt sein".

Man kann sich vielleicht noch vorstellen, dass ein Vater Lehrgespräche mit einem Sohn oder Schüler schriftlich niederlegt; niemand aber wird glauben, dass Szenen wie die folgenden erlebt und von AVICENNA selbst niedergeschreben sein können :

ABUALI will (S. 80), dass ABUZALEM, nachdem er die Härtung des Quecksilbers gelernt hat, nun auch das Sublimieren lerne. ABUZALEM füllt zwei Pfund Quecksilber in die Aludel und heizt an. Nach einiger Zeit kehrt er zurück und nimmt den Deckel ab; dabei spritzt ihm das Quecksilber ins Gesicht, so dass er Brandwunden davonträgt und im Zorn darüber seinen Vater vor allen Anwesenden beschimpft. Der Onkel tritt hinzu und gibt ihm Belehrung über die drei Arten der Sublimation. ABUZALEM schimpft nun über beide, weil sie daran Schuld seien, dass er sich verbrannt habe. ABUALI nennt ihn einen Idioten und ein Hundegesicht, worauf er erwidert : Wenn ich ein Hund bin, bin ich auch der Sohn eines Hundes, und wenn ich schlecht bin, bin ich nicht aus der Art geschlagen. Willst du mich etwa prügeln ? ABUALI ruft : Zweifelst du daran ? Verlass dich darauf, ich werde dich prügeln und umbringen ! Gleich darauf machen sie Frieden. ABUALI bittet den Sohn um Verzeihung, schickt ihn in sein Haus „*et docuit eum sublimare Mercurium sicut dicemus in hoc libro*".

Auch eine andere Szene (S. 318) möchte ich noch anführen, in der ABUZALEM seinen Vater fragt, woher er nur seine hohe

Weisheit habe. ABUALI antwortet : „Das alles habe ich gelernt durch vieles Lesen und wenig Schlafen, durch wenig Essen und noch weniger Trinken. Soviel meine Genossen für Licht ausgegeben haben, um nachts Wein zu trinken, ebensoviel habe ich für Öl ausgegeben, um zu wachen und nachts zu lesen. Soviel sie für Gelage ausgegeben haben, so habe ich noch mehr für Licht ausgegeben, um zu wachen und nachts zu studieren. Hätte ich das nicht getan, so wüsste ich nichts vom Magisterium".

Dass AVICENNA eine ungeheure Arbeitskraft besass, beweist der Umfang seiner literarischen Leistung; aber dass er sich, während andere die Nächte durch tranken, der strengsten Enthaltsamkeit befleissigte, konnte nur jemand schreiben, der von AVICENNAS wirklichem Lebenswandel nichts mehr wusste.

Wertvollere Aufschlüsse über die Quellen des Buches und den persönlichen Standpunkt des Verfassers dürfen wir da erwarten, wo er sich mit den Naturphilosophen und Alchemisten der älteren Zeit auseinandersetzt. Es lassen sich dabei drei Formen von Bezugnahmen unterscheiden : die gelegentlichen Anführungen alter Autoren, die Auseinandersetzungen mit den arabischen Meistern der Alchemie und die Verzeichnisse von Alchemisten, denen keine sachlichen Erörterungen angeschlossen sind.

Die Seltenheit griechischer Namen ist ein Zeugnis für den weiten Abstand des Buches von den alten Quellen. „Homines qui loquuntur, dicit PYTHAGORAS" ist alles, was S. 291 von dem Vater der *Turba* gesagt wird. Ob sich hinter MICRATIS philosophus S. 221 MICRERIS, DEMOKRITOS oder SOKRATES verbirgt, kann ich nicht feststellen. Ebensowenig ist LACONIA S. 297 oder LANCENIA und DANISA S. 73 festzustellen. So bleiben nur noch die Erwähnungen von PLATO, ARISTOTELES und HERMES übrig. Der Name des HERMES erscheint in der Form ARAMUS, einmal auch als HARAMUGE S. 55; eine Geschichte, in der er seinen Sohn auf die Schulter setzt, wird S. 46 auch von PLATO erzählt (23). ARISTOTELES wird etwa sechsmal erwähnt, aus dem arabischen *Steinbuch* wird S. 45 einiges angeführt.

---

(23) Die Einleitung des Stücks ist ganz verdorben : ' Et Plato dicit, *Plato pater unum de lo Salomares*, quod quaesierunt ad illum *alamure*, si erat verum magisterium.' Da *alamure* offenbar *al-umar'ā*, die Fürsten bedeutet, ist auch *de los alomares* zu lesen. Die Eingangsworte sind nach der alten Hs. der Hunterian Library in (Et) Plato dicit *per* unum... zu verbessern.

Nach alter Überlieferung war KHĀLID IBN YAZĪD der erste muslimische Alchemist; als sein Lehrer gilt der Mönch MORIENUS, nach anderen STEPHANUS von Alexandria. Die Gespräche des MORIENUS mit dem König CALID sind uns bisher nur in lateinischer Bearbeitung bekannt und müssen in Spanien hohes Ansehen genossen haben (24). Der Verfasser des Avicennabuches nennt MORIENUS in Cap. VI der ersten Dictio einen homo regis VERSUSAHA, es ist aber unmöglich, ohne handschriftliche Varianten etwas über diesen Namen zu sagen (25). Vom Inhalt der Schrift *De Compositione Alchemiae* scheint der Verfasser nicht viel zu wissen, jedenfalls steht das, was er über die Lehren des MAURIENUS erzählt (26), nicht in diesem Buch und setzt die Bekanntschaft mit der Alchemie des JĀBIR voraus. Man muss also annehmen, dass noch andere MORIENUS-Schriften im Umlauf waren, oder dass der Verfasser seine Polemik auf eigene Erfindung baut.

Zahlreich sind die Stellen, an denen GEBER ABENHAEN, also JĀBIR IBN ḤAYYĀN genannt wird, doch enthält Kap. III, S. 49-52 alles Wesentliche. GEBER gilt als „Magister Magistrorum in hoc magisterio", wird aber gleichwohl als ein Mann hingestellt, der durch unverständliche Allegorien und dunkle Reden die Herzen der Weisen zu verwirren trachtete. Der Verf. weiss, dass GEBER das Elixir auch als Talisman und als Arznei verwendete, und er muss einige Kenntnis vom *Buch der Siebzig* gehabt haben, nennt aber nirgends den Titel einer echten GEBER-Schrift (27). Im Ganzen gilt auch für den grossen GEBER, dass man nicht ihm glauben soll (28).

Wesentlich günstiger lautet das Urteil, das der Verf. in seiner Rolle als ABUALI ABINCINE *„de Babylonia"* über JAHIE ABINDINON, d.i. YAHYĀ IBN DHI'N-NŪN fällt. Dieser hat klar und deutlich gelehrt, was GEBER dunkel sagte, und er hat die Stoffe mit ihren richtigen Namen bezeichnet. Sein Hauptwerk scheint ein *liber*

(24) Vgl. J. RUSKA, *Arabische Alchemisten I. Chālid Ibn Jazīd Ibn Mu'āwija.*

(25) Er müsste dem HERIZARTEM, d.h. HERAKLEIOS der gedruckten Übersetzungen entsprechen. Vgl. *Ar. Alch.* I, S. 44.

(26) Im Druck falsche Seitenzahl 61 vor 64.

(27) S. 473 wird eine Schrift *Lumen Luminum* von GEBER empfohlen, die im Codex Speciale der Bibl. Comunale zu Palermo (Rivista Sicula), Bd. VII, 1872, S. 60 unter Nr. 26) enthalten ist. Das Buch wird meistens RASIS oder AVICENNA zugeschrieben.

(28) Dictio II, S. 96 : ' Et hoc dico, quod non credas Gebero nec Maurieno.'

Sponsalitii gewesen zu sein. Leider wissen wir nichts über Heimat und Lebenszeit dieses Mannes; dass der Verf. an den Mystiker und Alchemisten DHU'N-NŪN AL-MIṢRĪ gedacht hat, ist kaum anzunehmen.

Auch über RASIS wird nur Lobendes gesagt. Er war ein scharfsinniger Philosoph, schrieb viele Bücher über Philosophie und Alchemie und sagte die Wahrheit ohne Verdunkelung : „Si tu invenies de libris suis, lege eos, quia multa bona invenies ibi"... Was ihm nachher über die Härtung des Quecksilbers, die Änderung der Naturen, die Vermischung der beiden Steine *petra naturalis* und *herbalis* sowie die Hinzufügung eines dritten nachgesagt wird, ist jedoch weder dem Wortlaut noch dem Sinne nach in den echten Schriften zu finden.

Den breitesten Raum widmet der Verfasser der Auseinandersetzung mit ABIMAZER, (d.h. ABŪ NAṢR) AL-FĀRĀBĪ : „Dieser gehört zu den Grossen der Kunst, er hat vielen Blinden die Augen geöffnet und viele Siegel gelöst : wie sollten wir also übles von ihm sagen, da wir doch alles, was wir wissen, von ihm empfangen haben? Es gibt keine besseren Bücher als die seinen, und man muss sie studieren, wo immer man sie finden kann, wenn er auch nicht alles gesagt hat, was man wissen muss". In den Büchern über den Anfang der Welt, wo er von den sieben Planeten spricht, fragt er auch, was es jenseits der Natur gibt, und antwortet : es ist der Stein, den die Philosophen verbergen und den sie auf drei Arten bezeichnet haben. Unter *petra herbalis* sind die Haare, unter *p. naturalis* die Eier, unter *p. animalis* das Blut zu verstehen. Dies ist wie alles, was der Verfasser von besonderen Lehren und Vorschriften AL-FĀRĀBĪS erzählt, ebenso erfunden wie das Meiste, was er über die anderen grossen Alchemisten zum Besten gibt.

In Kap. VIII, das sich mit den Leugnern der Kunst befasst, werden keine Namen genannt. Es ist dadurch merkwürdig, dass es am Schluss (S. 75) eine lange Aufzählung der 23 Wunderkräfte eines Elixirs bringt, das den Namen *fortitudo saeculi*, also arab. *quwwat al-'ālam* führt. Ich lasse hier den Text folgen; vieles davon ist unverständlich und wird sich nur mit Hilfe von Handschriftenstudien klären lassen.

„Et modo dicam tibi valetudines fortitudinis : et sunt viginti tres. Indurare, mollificare, incerare, fermentare, sponsalitium in

minima calce, incolorare et subtiliare : fortitudinem ignis percremare cum alia re per *alcofol*, per *desamar*, per auferre timorem, per daemones conjungere, per malum quod faciunt animalia quae mordent, per canem rabidum, per coecos illuminare, per claudos sanare : cum hoc comedunt daemones, per sanare lepram, quae est de melancholia, per siccare colorem, per facere ad *castoson*, per colligere ut non siccentur, per dulcescere aquam, per solvere ferrum, per dormire, per ydropicum ex frigiditate sanare, per rugitum aurium auferre, per siccitatem nervorum, per omnem infirmitatem frigidam et siccam".

Eine ganz andere Gruppe von Alchemisten lernen wir in Dictio VI S. 312-315 kennen. Sie treten ohne erkennbaren Grund in einem Kapitel auf, das nach der *Tabula Dictionum* von der Präparation des Messings, nach der Überschrift im Text „De latone et de verbis dialecticis et physicis" handelt. Dass unter *allaton* oder *lato* das Messing zu verstehen ist, ergibt sich aus dem ersten Abschnitt des Kapitels (29). Auf die Einzelheiten seiner Präparation, Waschung, Erweichung, Sublimation und Lösung kann ich nicht weiter eingehen; die Paragraphen, in denen die Namen der Alchemisten auftreten, sind ohne ersichtlichen Grund zwischen die Sublimation und die Lösung eingeschaltet und der Text besteht aus Fragen und Antworten, die auf eine einheitliche Quelle schliessen lassen. Ich beschränke mich darauf, die Namen anzuführen; sie sind vorerst nicht zu deuten und müssen, wenn sie nicht fingiert sind, einer Generation maurischer Alchemisten angehören, von der wir sonst keine Nachrichten haben.

S. 312 : Dixit HAUM :—könnte aus HASAN entstanden sein.
Dixit CUZAHIR :—ABŪ ẒĀHIR oder QUSAIR?

S. 313 : Dicit LUBEIT :
Dicit FARAFFAR fil. de AHEMAR ABUALHABEIT :
Dicit HAURABUA :

S. 314 : Dicit ALMUGIER ABLIALMO HIRET
Dicit ALMOCARRAM :
Dicit ALHOCARRAM ABUALA GEGG
Dixit XEHEIR :

(29) Die Herkunft des Wortes ist noch ungeklärt. Vgl. J. RUSKA, *Arab. Alch.*, I, S. 45.

S. 315 : Dicit Iahie Abengalid Abnedenon :—siehe S. 17.
Dicit Abubir Abenhanair, Abancere, Abensade, Abenharaula :

Auch in Kap. XV, das vom Zinn handelt, treffen wir einige Namen an, doch bilden sie keine geschlossene Folge :

S. 290 : Dixit Hahomar Abnabi Rabiha :
S. 291 : Dixit Abugael und dixit Abugalid :
S. 292 : Dicit Abulbagezar Alhalci :

Vielleicht ist Abulbagezar der als „Elbo interfector" aus Morienus *De Compositione Alchemiae* bekannte Autor, dessen Namen ich *Arab. Alch.* I, S. 44 als Abū'l-Jazzār wiederhergestellt habe; etwas Sicheres lässt sich über den Autor auch heute noch nicht sagen.

Ich wende mich jetzt jenem berühmt gewordenen Verzeichnis der Alchemisten zu, das in Dictio I am Schluss von Cap. VI eingeschoben ist und im wesentlichen schon Vincentius Bellovacensis bekannt war. Wir verdanken Berthelot eine ausgezeichnete Analyse des Kapitels und die Feststellung der lateinischen Autoren (30). Eine Photographie der entsprechenden Stelle aus der alten Handschrift der Hunterian Library zu Glasgow (31) ermöglicht es mir, das Verständnis der Namen und Zusammenhänge einige Schritte weiter zu fördern.

Ich gebe den Text nach dem Druck S. 66 in drei Abschnitten. Die von Berthelot a.a.O. S. 301 mitgeteilten Varianten sind mit B, die der Handschrift der Hunterian Library mit H bezeichnet.

I. Modo dicam (H : et modo dicamus) tibi fili Abuzalemi, qui fuerunt magistri, quos ego audivi aut vidi; et sunt usque in hodiernum diem, a quibus multa poteris addiscere, et elige ex eis bonos sive malos : Adam. Noe. Iariz (B : Idriz H : Iaruch). Aramus (B : Moyse? H : Atamuz). Squiliarupiz (H : Siquiliampiz). Leuf. nullum prophetam (H : nullam prope feminam) dicam tibi. Rex Galud (H : Reggalud) de Babilonia. Rebilogar (H : Rebilegat) bebazuria (H : de Baruria). Isaac Judaeus de Moiros (B : Demones H : De uiones). Ombre abelhata. alias Abnabitalui (H : Alie ahaua bitalium).

Die Liste der alten Propheten ist ziemlich lückenhaft; das

(30) *La Chimie au Moyen Age*, Bd. I, S. 300-303.
(31) D. W. Singer, *Catalogue*, Nr. 123, S. 112.

von mir im Islam Bd. XVIII, 1929, S. 293 veröffentlichte Verzeichnis nennt auch Seth, Abraham, Jakob, Moses, Josuah, David und Salomo. Der Name JARIZ ist selbstverständlich aus IDRIZ, d.h. IDRĪS (oder HENOCH) zu erklären. ARAMUS scheint BERTHELOT für eine Entstellung von MOSES gehalten zu haben; der Name bezeichnet sicher den älteren HERMES. Mit SQUILIARUPIZ und LEUF, die offenbar auch noch in die Reihe der ganz alten Autoritäten gehören, kann ich nichts anfangen, auch wage ich keine Entscheidung, ob die nachfolgenden Worte in der Handschrift H besser erhalten sind als im Druck. GALUD ist GOLIATH, aber was REBILOGAR bedeutet und ob etwa RABBI LAZAR zu lesen ist—*g* und *z* werden leicht verwechselt—muss ich dahingestellt sein lassen. BERTHELOT scheint Bebazuria für eine Entstellung von BUBACHAR gehalten zu haben, es gehört aber offenbar zu REBILOGAR als dessen Bezeichnung nach einem Lande; nur scheint mir Azuria, also Assyrien, trotz der Nähe von Babilonia unsicher. Die Lesung B *demones* halte ich für falsch, aber de Moiros könnte wohl „von den Mauren" bedeuten. Sehr interessant sind die beiden letzten Namen : unter OMBRE ABELHATA ist kein anderer als der Kalif 'OMAR IBN AL-KHAṬṬĀB, und unter ALIAS ABNABITALUI der Kalif 'ALĪ IBN ABĪ ṬĀLIB zu verstehen.

II. Et ante istos alii gentiles : Aramus (H : Aramuz) medianus. et alius ultimus. Zucrat (H : Et verat). Bucras (H : Buerats). Flaton. Claron (H : Caton). Pliuus (H : pulvis) Zaib (H : et alii). Zubaibar (H : et Vbaibar). Aristoteles. Alexander. Costa fildeluca (H : filius de Luca). Batlanunz (H : Batlanuts). Geber, Aben Haen (H : abenha est). Abimezer (H : Abu mezer). Alfarabi (H : alpharabi). Jahie, Abendenon (abedenon). Mahomet (H : Mafomet de Razi). Pizagros (Pizagors). Nitafors Abul, Caradisse. Maurienus. Jacob Almonum. Abumazar (H : Abu Nazar). Albateli. Alaua, Rezini (H : Alhauarezmi). Cotahiua (H : Tochauia). Higer. Geben (H : Geber) et multi alii quod (H : quos) tibi dicere non potui.

Die Erwähnung eines mittleren und letzten ARAMUS sichert die Deutung auf HERMES (32). Im Druck ist öfters das Zeichen für *et* mit Z verwechselt, in ZUCRAT liegt die Sache aber umgekehrt,

(32) Vgl. J. RUSKA, *Zwei Bücher De Compostione Alchemiae und ihre Vorreden*, Archiv f. Geschichte der Mathematik, der Naturwissenschaften und der Technik, N. F. II (1928/29), S. 28 ff.

hier hat H den Fehler. Natürlich ist SOKRATES gemeint, wie BUCRAS den HIPPOKRATES bezeichnet. In CATON bezw. CLARON vermute ich ZENON, in PLIUUS, das von H missverstanden ist, den BALĪNĀS, d.h. APOLLONIOS von Tyana, jedenfalls nicht den PLINIUS. ZAIB scheint aus „et alii" entstanden zu sein, ZUBAIBAR oder VBAIBAR kann ich nicht erklären. BATLANUNZ ist PTOLEMAIOS, PIZAGROS und NITAFORS sind Entstellungen von PYTHAGORAS (33); ob in CARADISSE der Name KRATES steckt, ist ganz unsicher. ALMONUM könnte AL-MA'MŪN bedeuten. ABUMAZAR ist der grosse Astronom ABŪ MA'SHAR, der in den lateinischen Übersetzungen gewöhnlich ALBUMAZER heisst, ALBATELI muss AL-BATTĀNĪ sein, und in den beiden Namen ALAUA REZINI verbirgt sich, wie aus H klar hervorgeht, (MUḤAMMAD IBN MŪSĀ) AL-KHWĀRAZMĪ. Für COTAHIUA und HIGER habe ich keine Erklärung gefunden.

III. De Christianis Iohan. Euangelista, Prior Alexandriae. Garsia (H : Gaṛcia) cardinalis. Gilebert (H : Guilebert) cardinalis. Hug. (H : Huc) apostolicus vel universalis (H : Guilelmus) archiep. Penus monachus. Durandus monachus. Virgil (H : Virgius). almortid (H : Almordid). Dominicus Egidius magister hospitalis Ierosolomitani, qui traxit librum de 125 lapidibus. Episcopus Antroicus Dominus de ponderibus. et iste episcopus docuit me magnum (H : om.) magisterium in Africa. et dixit, quod acciperem aurum vivum et sublimarem duabus vicibus. ut facias vivum, infunde illum (H : album) in sale : et mitte eum (H : illud) in stercore humano et maneat tibi (H : ibi) per 2. dies et (H : in) olla ferrea. Et in hoc magisterio est sensus magnus (H : magnus sensus). et apostolicus Dominicus docuit me super hoc duas (H : duras) res : unam ad indurandum et aliam ad incerandum, etc.

Zu diesem Teil des Textes verweise ich auf BERTHELOT, S. 301 ff. Seine Erklärung des Ausdrucks „Prior Alexandriae" aus „Jean le Grand prêtre, dans la divine Evagie" will mir nicht einleuchten; eine andere Erklärung kann ich aber selbst nicht vorschlagen.

Über zwei andere Namenlisten, S. 113 und S. 209, kann ich mich kurz fassen : sie bestehen aus Anhäufungen von Namen,

(33) Vgl. J. RUSKA, *Turba Philosophorum*, S. 24 und S. 26.

die so verstümmelt und entstellt sind, dass nur die wenigsten gedeutet werden können.

S. 113 : Dictio V, cap. I. Dixit Abuali Avincine : In hac dictione dicam radicem huius libri, et loquar talia, quae nunquam alicui revelata fuerunt : dicemus per deza, per habubede, per habuahabez, per xina, per achomar, per mozram, per zalema, per alhuaxi, per aelge, per nostro femorralab, abimaz alfarabi naturas specierum etc.

S. 209 : Dictio VI, cap. III. Dixit Abuzalemi : Iacob Alchinili, ad patrem suum Alguazir : Alagel Abuali Abincine, filius Hauz, filius Xad, filius Xada, per mulierem quae fuit de Auida.

ALCHINILI ist natürlich AL-KINDĪ; was die Listen im übrigen bedeuten sollen, und woher sie stammen, ist unmöglich auszumachen.

Weiter unten folgt noch : Dixit qui fecit librum author, haec est inquisitio Xad : est inventio Xada, cuprum Auida alexir : hoc est dicere, quod inveniet cuprum per alexir.

Über den sachlichen Inhalt des Buches, also die angewandten Stoffe, Apparate und Verfahren, lässt sich kein kurzer Überblick geben, weil der schrankenlose Gebrauch von Decknamen sichere Angaben erschwert oder unmöglich macht, und weil die Kapitel mit ihren vielen Unterabteilungen einen weit bunteren Inhalt haben, als die *Tabula Dictionum* ahnen lässt (34). Wenn z.B. Dictio V mit ihren 28 Kapiteln eine klare Disposition aufweist, wonach zuerst über die Metalle und Legierungen, dann über Markasit und Antimon, dann über die Salze, die Geister und gewisse Tierstoffe, zuletzt über Essig und Zitronen gehandelt wird, so wird niemand vermuten, dass z.B. das Cap. XIX, S. 146-151 nicht nur das Quecksilber beschreibt, sondern alle möglichen Fragen des ABUZALEM an seinen Vater enthält, die nicht zum Thema gehören. Ebensowenig würde man erwarten, dass ein Kapitel, das nach der *Tabula Dictionum* von dem „Metall" handeln soll, im Text aber die Überschrift *Quod filius biberit aquam frigidam mixtam cum fervento* trägt (35), eine Streitscene

(34) In Dictio VI, die aus 33 Kapiteln besteht, sind die Kapitel in zahlreiche Unterabschnitte geteilt; so enthält Kap. 15 nicht weniger als 46 und Kap. 19 sogar 53 Abschnitte.

(35) *Fervento* ist Druckfehler für *ferventi*.

zwischen Vater und Sohn darstellt und erst am Schluss ganz unvermittelt die Bemerkung bringt, „metallum faciunt duobus modis, et non sunt nobis necessaria aliqua e metallis in hoc magisterio". Der Satz ist ganz unverständlich, wenn man *metallum* im üblichen Sinne nimmt; man erfährt erst S. 150 aus einem Abschnitt des vorhin genannten Cap. XIX, dass das Wort eine Legierung aus Kupfer, Blei und Zinn bezeichnet. Dies wird auch durch Dictio VI, Cap. XVII, S. 318 *De maneriebus metalli* bestätigt. Da in der *Tabula Dictionum* der Titel des Kapitels „De praeparatione *asceni* sive *metalli*" lautet, muss der arab. Text die Namen *khār ṣīnī* und *ṭāliqūn* nebeneinander enthalten haben (36).

Ich lasse noch einige andere Auszüge folgen, die sachliches Interesse haben. Über die Herstellung des Alkalisalzes wird in Dictio V, Cap. XI, S. 134 folgendes berichtet :

„Alcali est herba quaedam, a qua abstrahunt succum, et faciunt inde sal, et dicitur *Sosa :* et est viridis cum magno folio in longitudine medii palmi, et spinosa multum. Faciunt foveam subtus terram, et extrahunt succum de ea et implent foveam de illo succo : et illa est *sosa*, et color eius cineritius lenis, sed non multum perforata (?). Tingunt inde pannos, et cum aqua eius et cum aqua de gallis tingunt nigrum etc." — Die Beschreibung der Pflanze lässt eher auf eine Distelart schliessen als auf Salsola oder Salicornia. Das Schwarzfärben mit Galläpfeln würde einen Gehalt an Eisen voraussetzen, für den sich nur schwer eine Erklärung geben lässt.

Eine andere Beschreibung der Gewinnung des Alkalisalzes findet man in Dictio VI, Cap. XXVIII, S. 380; ich stelle sie der vorhin mitgeteilten gegenüber :

„Sal alcali est herba quae vocatur *albellui*, et extrahunt succum ab ea, et implent foveas, et induratur, et efficitur lapis perforatus niger, et vocatur *sosa*, tantum quod cooperiatur, et dimittunt ad solem V dies. Postea accipe filtrum etc."

Vom Alaun erfahren wir S. 138, dass es zwei Arten gibt, *hiameni et magrani : hiameni* est album et stringit, et *magrani* est rubeum

(36) Die Bezeichnungen werden synonym gebraucht. Zur Etymologie von *ṭāliqūn* vgl. J. Ruska, *Chemie in Irāq und Persien im zehnten Jahrhundert n. Chr.*, Der Islam, Bd. XVII, 1928, S. 292.

et non intrat in nostro magisterio ad album. Der weisse wird benützt, weil er gut und gründlich reinigt, er findet aber auch beim Lösen, Sublimieren, Härten, Erweichen und anderen Arbeiten Anwendung. Nach S. 385 gibt es weissen, roten und gelben, doch wird der gelbe nicht verwendet. Der weisse entsteht am Meer „in parte dextra saeculi", d.h. auf der Südseite der Welt, und heisst eben danach *hiameni*, d.h. *al-yamanī;* der rote wird hier *almahagra* genannt, und ich vermute, dass in beiden Fällen das Wort *al-miṣrī*, der ägyptische, zu Grunde liegt.

Die Einwirkung von JĀBIRS *Liber Septuaginta* lässt sich sehr einleuchtend aus dem kleinen Kapitel XX von Dictio V *De capillis et eorum natura* (S. 151) belegen :

Capilli sunt in multis modis, de homine grandi, de homine parvo, de mediocri, de bestiis, sed de bestiis non intrant in hoc magisterio; et sunt ibi de pilis hominum, et sunt de colera nigra, de colera citrina, de flegmate et de sanguine. Et capilli, qui intrant in magisterio ad habendum lapidem, sunt (besser : sint) de iuvenibus de XIII annis, qui sunt de sanguine [et] de IV humoribus; et fumus capillorum reddit cuprum de colore auri".

Man braucht nur den Anfang des *Liber Divinitatis* nachzusehen, um die Quelle dieses Kapitels festzustellen (37). Als eine Fortbildung der Lehren JĀBIRS ist auch die Abhandlung *De Sanguine, et in quo tempore debeat colligi, et de quibus personis* zu betrachten, mit der Dictio VII beginnt.

Beschreibungen von Gefässen und Öfen sind an vielen Stellen eingeschaltet, ohne eine wesentliche Bereicherung des aus RĀZĪ Bekannten zu bringen. So sind Vorschriften für Aludeln und Rezepte für verschiedene Arten von *lutum magisterii* in Dictio VI, Cap. VI *De sublimatione corporum* eingefügt, die Herstellung von Capellen wird Dictio IV, Cap. II bei der Schmelzung des Kupfers, die von Doppeltiegeln Dictio VI, Cap. XIX beim Silber beschrieben. Vom Bau der Öfen handelt das erste Kapitel der Dictio VI und ein Abschnitt von Dictio VII, S. 401. Auch der Beschaffenheit der Gefässe wird viel Aufmerksamkeit geschenkt, insbesondere wird vorgeschrieben, welche Stoffe in gläsernen, irdenen oder metallenen Gefässen aufbewahrt werden müssen. Viele Namen

(37) M. BERTHELOT, *Archéologie et Histoire des Sciences*, S. 310 ff., vgl. auch ebenda S. 327.

von Gefässen bedürfen noch der näheren Bestimmung (38). Ein besonders interessantes Kapitel am Ende von Dictio I, S. 88 ff., handelt von der Herstellung von Gussformen für goldene Gefässe und Münzen.

Es ist bekannt, welche zentrale Stellung dem *'ilm al-mīzān*, der Wissenschaft von den Zahlenverhältnissen, in JĀBIRS System der Wissenschaften insbesondere in Anwendung auf die Alchemie zukommt (39). Als einen Nachhall dieser Lehre wird man es wohl betrachten dürfen, wenn unser Autor in seinem Buch immer wieder Zahlenangaben über Dimensionen, Gewichte, Zeiten und astronomische Erscheinungen bringt oder seltsame Zahlenspekulationen einschaltet, für die uns einstweilen noch der Schlüssel zum Verständnis fehlt. Ich möchte nicht darauf verzichten, den wesentlichen Inhalt eines Exkurses mitzuteilen, den der Verf. — unter AVICENNAS Namen — über sein eigenes System der Wissenschaften dem Cap. XV der Dictio VI, S. 287 ff. eingebaut hat.

Danach gibt es zehn Wissenschaften, die für das Magisterium notwendig sind; vier davon werden als Mütter, sechs als Töchter bezeichnet. In der Aufzählung ist die Unterscheidung nicht durchgeführt, doch ergibt sie sich nachher aus dem Text.

Die *Dialektik* ist notwendig, um zu wissen, ob es eine Alchemie gibt oder nicht.

Die *Geometrie* ist für das Magisterium nützlich, um die Grössenverhältnisse aller Dinge zu bestimmen, die für das Werk gebraucht werden.

Die *Naturwissenschaft* ist erforderlich, um die Naturen der Steine und aller anderen Dinge zu erkennen.

Die *Kenntnis des Firmaments* ist nötig, um die Zeiten der Wärme und der Kälte zu wissen.

Der *Algorismus* ist zur Berechnung aller Dinge notwendig.

Die *Arithmetik* ist notwendig „ut sciamus materiam diminuendi computum et ascendendi".

---

(38) Gewisse Etymologien im Text können nur vom Übersetzer herrühren. So wird S. 339 über *botus barbatus* gesagt: In totis magisteriis botum barbatum est verbum Graecum: *barba* apud illos est foramen, et *botus barbatus* vult dicere [pro] perforatus etc. Dies kann selbstverständlich nicht im arabischen Text gestanden haben.

(39) Vgl. P. KRAUS, *Studien zu Jābir Ibn Ḥayyān*, Isis, Bd. XV, 1931, S. 19, und derselbe, III. Jahresbericht des Forschungs-Instituts für Geschichte der Naturwissenschaften in Berlin, 1930, S. 25.

Die *Theorica* (in der Physik) braucht man, um die Grade der Wärme und Kälte, Trockenheit und Feuchtigkeit der einzelnen Dinge zu kennen.

Die *Musik* braucht man, um die Laute der Dinge zu verstehen.

Die *Astronomie* ist notwendig, um zu wissen, zu welcher Stunde man mit einer Operation beginnen oder aufhören soll.

Die ersten vier Wissenschaften sind die Mütter, die übrigen die Töchter; die fehlende zehnte Wissenschaft muss die vorher schon erwähnte Philosophie sein, „quae habitat totum mundum".

Man sucht nun freilich vergebens nach Spuren tieferer Kenntnisse des Verfassers, denn wenn auch da und dort einmal eine Bemerkung auftaucht, die eine gewisse Beschäftigung mit Astronomie, Geometrie oder Zahlenlehre dartut, so ist der Inhalt doch meist so primitiv und noch öfters so unsinnig, dass man sich überwinden muss, um weiter zulesen.

Astronomisch ist von einigem Interesse die Erwähnung des Astrolabs in dem Satze „fac de eo circulum rotundum sicuti folium de foliis *astrelabri*" S. 140, dann der merkwürdige Ausdruck *ascende astrolabium cum horis* S. 201 und das astronomische Kapitel, mit dem der Verf. Cap. I von Dictio VII einleitet, um genauere Angaben über die Zeiten machen zu können, in denen das Sammeln des für das Magisterium erforderlichen Blutes am günstigsten ist. Hier wird die Anwendung des Astrolabs wie folgt vorgeschrieben : „Exalta astrolabium de hora illa quando intrat Sol in primo gradu de prima facie tauri, et de primo gradu de postrema facie Virginis usque ad postremum gradum de prima facie Scorpionis; est tempus magis aequale omnibus aliis temporibus".

Zeitangaben kommen fast in allen grösseren Kapiteln vor. Sie erstrecken sich nicht nur auf Jahre, Monate und Tage, sondern oft auch auf Stunden, Minuten usw., die in den Kapiteln über die Lebensdauer der Dinge bis ins Unendliche weitergeteilt werden. Wer will, mag die Berechnungen als ein Seitenstück zu den Berechnungen über den Atomzerfall betrachten, die die heutige Physik anstellt. Einige Zeilen aus dem Kapitel *De Vita alexiris Solis*, S. 184, mögen die Angaben unseres Verf. kennzeichnen :

„Modo dicam tibi planius : homo usque ad L annos crescit in totis viribus suis, et in corpore suo usque ad XXIV. Equus crescit viribus suis usque ad XII annos, et in corpore suo usque

ad V... Mercurius vivit L annos, et sunt inter cuprum et petram, et Mercurium CCXXXV anni. Minuit in combustione ignis viginti quinque dies, et viginti quinque horas, et totidem puncti (40), et totidem secundi, et totidem tertii, et totidem quarti, et totidem quinti, et totidem sexti, et ita usque non habent finem" etc.

Geometrische Betrachtungen sind besonders in Cap. II von Dictio VI zu finden. Dass sie schon im arabischen Original enthalten waren, sieht man besonders deutlich in dem Abschnitt *Sub nomine calcis de vero lapide Geometrice et obscure*, in dem ziemlich alle Namen der arabischen Buchstaben zur Bezeichnung von Punkten in Figuren angeführt werden (41). Ich teile einige Sätze mit, um zu zeigen, dass auch hier ein Urteil über Sinn und Wert des Textes erst auf Grund einer neuen kritischen Ausgabe möglich sein wird.

„Omnes Seniores de Geometria dicunt, quod regula de medio est circulus, tres partes de ea, et septendecim partes de novendecim, de uno de triginta : et dicunt quadranguli. Quid est quadrangulus? Quadrangulus est, quod habet computa in sua spatula tot, quot habet in una quaque aliarum spatularum. Verbi gratia per Geometriam fac circulum, et scribe in centro *alef*, et in parte ubi possunt (lies : ponunt) compassum, scribe *gim*, et immedietate (lies : in medietate) *b*. ergo tantum est de *alef* ad *gim*, quantum de alis (lies : *alif*) ad *b*. Et scribe in quarta parte circuli *del* " usw.

Erwähnenswert ist auch die Aufzählung der Masse, mit denen in der Geometrie gemessen wird; ich teile den Anfang mit, weil er einige arabische Worte enthält, die ich noch nicht habe sicherstellen können : Maneries, cum quibus mensurant in Geometria, sunt quinque, scil. digitus, et *algerab*, et canna, et *albeu*, et *algazeb*. Digitus est duo ossa digiti, *algerab* est sex pilli iuncti infra tres. canna est XIV digiti : *algazeb* est mons (42), qui habet duodecim portas, unaquaeque porta habet M. et D. L. cannas, unaquaeque canna de quatuordecim digitis : ergo in illo monte unaquaeque porta habet XX.M et M. et DCC. digitos, et unusquisque digitus

(40) *Puncti* ist eine ungewöhnliche Übersetzung des arab. *daqā'iq* ' Minuten.'

(41) Sie sind S. 200 in folgender Weise verwendet : ' Pone per petram *alef*. et per quatuor elementa *gim. b. del. he.* et propter spiritum *zai.* et per septem sales. *ha. tha. hie. chef. lem. mum* (lies : *mim*). *non.* et per frumentum (?) et corpus et Mercurium *schin. hai. se :* et per totum aliud *zad.*'

(42) Verwechslung von *ḥabl* Ḥān, Seil mit *jabal* Berg?

habet octodecim pillos iunctos. Ergo sunt in isto monte de pillis iunctis CCM. millia renumerati quatuor vices, et D. M. renumerati III vices, et XXX milliaria et XXV.

Als Zeugnis für die ursprünglich arabische Sprache des Buches ist auch noch eine Stelle in Dictio V, Cap. XXIV von besonderem Interesse, in welcher S. 155 nach der Beschreibung der Arten und Anwendungen des Glases das „temperamentum spiritale" an der Musik erläutert wird :

Modo dicam tibi temperamentum spiritale. Tu scis quod dicit musica : quia voces sunt duodecim, et unaquaeque illarum dividitur in multis partibus. Talis est quod est *alexir*, *almagne* et *alibem*, et *alhanzar*, et *albenzar* et *hatin* (et) *aharta minalcaf;* et scias quod *azir* habet sonum altum, *albane* habet sonum, sed non altum; *aliben* habet sonum latum, *alhanzar* habet sonum longum. De vitro quod est bonum accipe.

Von den kursiv gedruckten arabischen Worten lassen sich die meisten nach der in den *Mafātīḥ al-'ulūm*, ed. Van Vloten, S. 240 ff. enthaltenen Sammlung musikalischer Termini wieder herstellen (43). Sicher ist zunächst *alhanzar* = *alkhinṣir*, der kleine Finger, und *albenzar* = *albinṣir*, der Ringfinger; möglich ist auch, das *aharta minalcaf* die Wiedergabe von *al-wusṭā min al-kaff*, „der mittlere (Finger) von der Hand" sein soll, während für den Zeigefinger *al-ṣabbāba* kein Äquivalent zu finden ist. Für die Bezeichnung der Töne ist *alibem* und *aliben* als Umschrift von *al-bamm* und *azir* bezw. *alexir* als Umschrift von *al-zīr*, gesprochen *azzīr* anzusehen. Wahrscheinlich ist dann *almagne* und das stark abweichende *albane* Wiedergabe von *almatnā* und *hatin* die von *ḥādd*. Der eigentliche Sinn der Stelle bleibt allerdings trotz dieses Deutungsversuchs unklar, und man hat den Eindruck, dass schon der Übersetzer seinem Text ratlos gegenüberstand.

Zu der ganzen Mannigfaltigkeit des Inhalts, die aus den beigebrachten Proben sichtbar wird, tritt nun als ein wesentliches Element noch die Menge der Decknamen und der fremdartigen allegorischen Beschreibungen chemischer Vorgänge (44). Sie

(43) Vgl. auch E. Wiedemann, *Zur Geschichte der Musik*, Beitrage z. Gesch. d. Naturw. LXVI, Erlangen 1922.

(44) Von ihrer Häufigkeit geben allein schon die Bezeichnungen in den Überschriften ein Bild. Gewöhnlich heissen die Decknamen nomina oder verba

beherrschen die Darstellung von Anfang an in einem Ausmass, wie ich das sonst in keiner anderen Schrift, auch die *Turba* nicht ausgenommen, gefunden habe. Das Verständnis der betreffenden Stücke wird dadurch noch besonders erschwert, dass die älteren und bekannten Namen vielfach durch unerhörte Neubildungen abgelöst werden, für die uns einstweilen die Gründe und Zusammenhänge verschlossen sind.

Schon das erste Kapitel der *Porta Elementorum* versetzt uns mitten in diese seltsame Welt, wenn es ankündigt, dass die vier Elemente „diversis nominibus figurantur", und dass „ibi etiam fit mentio de muscis, de scarabeis, de formicis, et de numero : de alchyzera, de fumo magisterii, de gallinis, de tractis, de aquila et bufone". In der ersten Allegorie soll farina sicalis, melii, hordei und tritici zu Broten verarbeitet werden, worunter nach einer späteren Erklärung Erde, Wasser, Luft und Feuer, nach einer andern S. 327 Blei, Blut u.a. zu verstehen sind. In zahlreichen anderen Fällen aber sucht man vergebens nach einer Deutung der Rätselworte. Einige Beispiele mögen veranschaulichen, was dem Leser alles zugemutet wird.

S. 76 : Et modo dicam tibi rem secretam : oculus hominis, oculus tauri, et de vacca, et de gallina, et de cervo. Et hominis oculus est ad significandum Mercurium, stercus humanum, et alia omnia stercora; est dens (et) lingua hominis, et aliae linguae (sicut ego exponam), sunt cera nigra et alba et rubea, et istae cerae sunt de capillis, et ovis, et sanguine. Ova nigrae gallinae sunt aqua de petra nigra, i.e. de capillis; aquila et grifus sunt lapides nostri : auripigmentum, ignis et sal. Ignis similiter, et in quibusdam locis terra, aurum vivum aqua : aliquando aër, et secundum diversa loca diversa est terra : et in hoc intellectu est necessarius magnus sensus : bletas, berdolagos, lactucas, spinachia, coriandrum, herba sana, multae earum significant lapides. Intellige quod dico : quia verum est, quia levamen est fermentum auri et argenti, et non est aliud levamen. Illud animal quod vocatur Anoca non intrat in magisterium etc.

S. 96 : Habeas de carnibus, scilicet de [de] porco, arietina,

figurata, simulata, secreta, obscura, obscure dicta, allegorica, oder die Überschrift besagt, dass im folgenden Abschnitt aenigmatice, parabolice, geometrice, obscure, philosophice von den Stoffen und Operationen gehandelt werden soll.

gallinacea, asinina, anatina, leporina, arogrilli, et comede de qua vis : et habeas panem triticeum, ordeaceum, panem siligineum, et panem milii : hic est opus alexir si tu intelligis. Sed hoc est verbum singulare.

S. 203 : Nares tortas fac rectas, et grossas fac tenues, et longas fac curtas, et habebis magisterium.

S. 214 : Quando vides filium tuum *azoch* transire, accipe urinam suam et distilla : et stercus suum, et mitte subtus fimum : et divide capillos suos per quatuor elementa, et iugula eum et sanguinem divide per quatuor elementa... et si vis facere rem utilem, quando nascitur filia in domo tua, in ipsa nocte iugula eam etc.

S. 304 : Modo dicam tibi qui sunt lapides nostri et quot sunt cum radice Philosophiae et sensu magisterii : et nominabo tibi in lingua de bestiis et de volatilibus et scorpionibus, sicut debeo facere filio meo, et non celabo tibi verbum... Et elementa se dividunt ab istis lapidibus, et sunt XIV : elephas. ursus. porcus. simia. bufo. tartuca. scorpio. aranea. lupus. cocolata. rana. piscis. grus. aquila. *algerid*. et omnia sunt XIV. lapides, sed dicunt quod cocollata et *algerid* unum et idem est.

S. 418 : Accipe de caulibus, et de portulacis, de bletis, de spinachiis, de ciceribus, de lenticulis, de arbellis, de tritico, de hordeo *orrez*, de fabis, de aluus (aliis?) coriandrum, allea (lies : allia), cepas, acetum, vinum, amigdalas, zafaram, napos, zaphanarias, cucurbitas, cucumeres, pimpinellos, de unoquoque pondera aequalia; da ignem per diem et noctem, et quando vis comedere, cum parvo cocleari accipe et comede, et nihil aliud comedas, et bis comede de isto in die, si vis habere magisterium : et dum comederis hoc, potes accipere tuas superfluitates duobus modis, quando vis mittere in aliqua parte barcam tuam, scinde ei nares, ne feriat in alia barca. Et quando intras in mari, et inveneris delphinum, mittes in os eius manum, et trahes eum, si teneas anulum istum in manu tua. Et dic (lies : hic) est anulus : Accipe de auro unciam 1. et fac filios, et in illa hora quando facies anulum, iugula eos, et iacta sanguinem super anulum nullo te vidente, et te anulo illo tenente occides delphinos. Quando dixi tibi de portulacis, intellige totam rem frigidam et humidam, sicut aqua pro caulibus rem siccam et frigidam, sicut terra, et ita intellige de omnibus aliis. Nisi esset propter dilationes, ego exponerem, sed credo quod intelligas. Et modo dicam tibi maneries alexiris.

Es ist auffallend, dass von den wenigen Decknamen, die Rāzī noch zur Bezeichnung der vier Geister benützt, nicht ein einziger im *liber de Anima* vorkommt, denn auch *aquila*, das als Übersetzung von *'uqāb* den Salmiak bezeichnet, wird in *De Anima* in anderem Zusammenhang gebraucht, während der Salmiak „dens diaboli" heisst (45). Wir haben es also bei diesen Decknamen und Allegorien mit jungen Wucherungen zu tun, deren Wurzel wahrscheinlich in Spanien selbst, jedenfalls aber im Westen zu suchen ist. Leider kennen wir von der Literatur der Alchemie in den westlichen Ländern des Islams noch viel zu wenig, um nach rückwärts Verbindungslinien ziehen zu können.

Ich wende mich nun einigen anderen in alten Sammlungen gedruckten AVICENNA-Schriften zu, die, wie sich zeigen wird, keine Übersetzungen aus dem Arabischen, sondern lateinische Originale sind.

Die erste ist die im Theatrum Chem., Bd. IV, (1659) S. 875-882 zwischen der „Epistola ad Hasen Regem" und „De Congelatione' abgedruckte *Declaratio Lapidis Physici Avicennae filio suo Aboali* (46). Der fehlerhafte Titel, in dem ABOALI, die Kunja des AVICENNA, als Name des Sohnes verwendet wird, spricht schon von Anfang an gegen die Echtheit. Aus dem Inhalt ist der Verfasser nicht sofort zu überführen, weil er vorsichtig genug ist, keinen seiner „Philosophi" und „Sapientes veteres" beim Namen zu nennen. Achtet man genauer auf den sprachlichen Ausdruck, so sieht man bald, dass hier, abgesehen von möglichen Anleihen, keine Übersetzung, sondern eine lateinische Originalarbeit vorliegt. Schliesslich entdeckt man auch eine unzweifelhafte Abhängigkeit von den Decknamen und Gedankengängen, die der *Turba Philosophorum* eigentümlich sind. In der Zusammenstellung, die ich hier nach S. 881 folgen lasse, habe ich die aus der *Turba* zu belegenden Ausdrücke in Kursivdruck wiedergegeben :

„Nulla aqua corporibus in resolutione convenit, quae cum eis

(45) S. 309 : Et ego quandoque voco sal armoniacum dentes diaboli : ideo ut nullus me intelligat.

(46) Über die Handschriften vgl. M. STEINSCHNEIDER, *Europäische Übersetzungen* und D. W. SINGER, *Catalogue*, S. 117, Nr. 128. Im Elenchus Auctorum et Tractatuum ist richtiger *Lapidis Philosophorum* gedruckt.

non cogulatur permanente decoctione, et non est alia nisi aqua mineralis, viscosa, *munda* ut lachryma oculi, vel sicut *aqua roris* madii; non est aliquid in mundo, quod possit stare loco eius aut ipsius supplere vicem, et vocatur a Philosophis *aqua vitae*, *acetum acerrimum*, aqua fortior totius mundi, *aqua divina* et *aqua permanens*, postquam regitur maturando, quae tamen absque suo corpore, cum quo igne miti coniuncta est, et cum eodem facta unione, permanens esse non potest, quia sicut accidentia sine substantiis non valent subsistere, sic spiritus in ignis combustione non permanent, nisi fuerint fixis corporibus sociati. Ubi igitur Philosophi in suis scribentes libris de azoth, argento vivo, aqua Mercuriali, *aqua sulphuris munda*, aqua forti, *aqua viva*, aqua acuta et aliis humiditatibus corpora solventibus fecerunt mentionem, non sunt locuti de rebus vulgaribus simpliciter a natura datis, sed de humidis vaporibus, a lapide Philosophorum extractis ingenio, voluerunt intelligere dicentes etc.

Es erübrigt sich, die Anklänge an MORIENUS und die Entlehnungen aus der Literatur über die „scharfen Wässer" ins Einzelne zu verfolgen, auf die man überall stösst. Ich kann aber nicht daran vorbeigehen, zu zeigen, dass der Verfasser auch das Buch *De Anima* gekannt und für seine Einleitung benützt hat. Er hat dazu das Kapitel XVI aus Dictio V fast Satz für Satz wiedergegeben, im einzelnen aber bald zugesetzt, bald weggelassen, was ihm gerade gefiel. Die folgende Zusammenstellung liefert den Nachweis :

De Anima S. 141 : Et quando dicunt magistri : Accipe sulphur *et fac hoc, et hoc, et hoc :* scias quod sulphur non ingreditur in nostro magisterio, sed nominaverunt sulphur ad significandum per lapidem eorum : sicut multi qui significant per lapidem suum auripigmentum, *et alii marcasitam... et alii bestias dicunt :* et non dixerunt nisi ut absconderent magisterium.

Declaratio S. 875 : Fili mi, ubi Philosophi dicunt in libris suis, Recipe sulphur *et auripigmentum*, non intrat in magisterium nostrum. Sed nominarunt sulphur *et auripigmentum*, explicantes lapidem Philosophorum, in quo lapide sulphur et arsenicum Philosophorum latent sive occultantur.

De Anima : Et qui dicunt accipe lapidem qui non est lapis, et sulphur quod non est sulphur, de auripigmento quod non est

auripigmentum, non dicunt nisi ut abscondant magisterium : *et nullus illorum dilexit magis filium suum quam ego dilexi te :* quia illi non curant de nominibus : quia lapis eorum unus est.

Declaratio : Et quidem Philosophi dicunt, est lapis et non lapis, *et est vilis in plateis, et in viis eiectus pedibus hominum calcatur, et ab unoquoque paupere potest acquiri*, et est sulphur quod non est sulphur, et est auripigmentum, quod non est auripigmentum, *et est ovum gallinas, bufo, sanguis humanus, capilli*, et non dixerunt talia nomina, nisi quod absconderunt magisterium. Et scias pro certo, quod Philosophi non curaverunt de nominibus, nisi uno nomine...

De Anima S. 141 : Et veniunt stulti et operantur sicut inveniunt in libris suis, et non possunt attingere ad *profectum :* dicunt, quia non est verum magisterium : et quia dicebant quod philosophi erant mendaces, et non erat etc.

Declaratio S. 876 : Fili mi, nunc veniunt stulti *quotidie*, et operantur, sicut invenerunt in libris, *super sulphur et arsenicum... et per amalgamationes*, et non possunt pertingere ad veram *perfectionem, et non est mirum, quia ipsi idiotae non intelligunt mentem Philosophorum*, et semper dicunt, quod magisterium non est verum, et quod hi Philosophi *errant* (lies : erant) mendaces.

De Anima S. 141 : Ideo composui librum istum, et illuminavi caecos, solvi catenatos, et intelligi feci quod non intelligebant, et solvi ligaturas : et dixi tibi cum *virtute Domini* secretum philosophorum, et eorum intentionem.

Declaratio S. 876 : Ideo fili mi composui istud capitulum, et illuminavi caecos, et aperui *thesauros et* catenas, et intelligere faciam insipientes id quod non intelligebant, et solvi ligaturas *vocabulorum*, et dixi tibi fili cum *veritate Dei* secretum Philosophorum, et eorum intentionem.

De Anima S. 142 : Et ideo iurando (lies : mando) tibi, quod istum librum non venundes nisi filio tuo, et filius tuus filio suo, et de generatione in generationem, et detur sapientioribus : quia malum feci quod revelavi puritatem *Domini :* ideo non reveles, quia dico in veritate, quod nullus philosophorum cum aliis fuerunt operati nisi cum sanguine, et cum capillis, et cum ovis, et hoc est secretum illorum.

Declaratio S. 876 : Et ideo *mando* tibi, quod librum istum nemini des nisi filio, et filius tuus filio suo, et sic de generatione in generationem, et hoc sapientioribus illorum. Et scias fili mi, quod malum feci, quod revelavi tibi puritatem *doni* illius. Non revelabis *hoc sanctum laudabile opus*. Et dico tibi fili in veritate, quod Philosophi cum aliis rebus non fuerunt operati, nisi cum sanguine *capillorum* (lies : cum capillis), et ovis exemplariter etc.

Eine andere AVICENNA zugeschriebene Abhandlung ist der in *Artis Auriferae quam Chemiam vocant* Vol. I, S. 260-280 gedruckte *Tractatulus Avicennae*, der auch in MANGETS *Bibliotheca*, Bd. I, S. 626 ff. nachgedruckt wurde und nach FERGUSON, *Bibliotheca Chemica*, Bd. I, S. 60 mit dem *Tractatulus de Tinctura metallorum*, den schon SCHMIEDER in seiner *Geschichte der Alchemie* S. 98 erwähnt, identisch ist.

Die Schrift ist in acht Kapitel geteilt, in denen zunächst von den Eigenschaften des Quecksilbers und Schwefels, dann von Gold und Silber gehandelt wird, während sich die weiteren Kapitel mit der Darstellung des philosophischen Steins und seiner Projektion befassen. Was der Verfasser im ersten Kapitel über das Quecksilber sagt, ist in seinem ersten Teil ein Auszug aus RASIS *De Aluminibus et Salibus;* der Rest des Kapitels scheint eigene Leistung des Verfassers zu sein. Auch in dem von den Metallen, insbesondere von Gold und Silber handelnden Kapitel II ist ein Teil den entsprechenden Kapiteln des RASIS entnommen. Das III. Kapitel scheint vorwiegend vom Verfasser selbst herzurühren. Es beginnt mit dem Satz „Certum est omnem rem esse ex eo, in quod resolvitur", worin man eine Vorwegnahme von BOYLE erblicken könnte, wenn nicht die Umwandlung von Eis in Wasser und die Erzeugung der Metalle aus Quecksilber als Beispiele angeführt wären. Ich gehe auf den Inhalt nicht weiter ein, da ich die Quellen im einzelnen nicht feststellen konnte.

Das IV. Kapitel *De Extractione aquae ex terra* ist im wesentlichen ein Kommentar zur *Tabula Smaragdina*. Einige Sätze mögen als Beleg dienen :

S. 268 : Et ideo philosophus dicit : quod illud *quod est inferius, est sicut illud quod est superius*. Et ista divisio est necessaria *ad perpetranda miracula rei unius* scil. lapidis; quae pars inferior

est *terra quae dicitur nutrix* et fermentum, et pars superior est anima, quae totum lapidem vivificat et reviviscere facit.

S. 269 : Et sic *descendat in terram et (sic) recipit superiorem vim* sublimando *et inferiorem* descendendo, ut corporeum fiat spirituale sublimando, et cum est spirituale, fiat iterato corporeum descendendo. *Et sic habes gloriam* claritatis *huius mundi*, et *fugit a te omnis obscuritas* et omnis inopia et aegritudo etc.

Ich habe schon in meiner *Tabula Smaragdina* S. 194-195 nachgewiesen, dass der Verfasser hier den Kommentar des HORTULANUS abgeschrieben hat. Auch am Schluss von Kapitel VI wird noch ein Satz der *Tabula Smaragdina* zitiert.

Kapitel V enthält ausser Anführungen nach alten Philosophen, denen ich nicht weiter nachgespürt habe, S. 272 einige Hinweise auf die *Turba*. Der Satz von der „mutatio complexionum" geht auf den Sermo II des EXIMEDRUS zurück, den Ausspruch des BONELLUS „Intenso vero et nimio calore destruitur opus" habe ich nicht finden können; am nächsten kommen wohl der Stelle die Ausführungen in Sermo XXXII. Auch eine Stelle am Schluss von Kapitel VII S. 277 hat der Verfasser, ohne die Quelle zu nennen, der *Turba* entnommen : „In primo tempore, scil. hyeme, terra concipit. In secundo, scil. vere, producit herbas et flores. In tertio, scil. aestate, maturantur fructus. In autumno vero, i.e. quarto tempore, colliguntur fructus". Der entsprechende Text ist in Sermo XVII des CINON enthalten (47).

In Kapitel VI werden als Operationen, die zur Darstellung des Elixirs notwendig sind, die Sublimation, Solution, Putrefaction, Ablution, Coagulation und Calcination genannt, zu denen S. 276 noch die Destillation, Descension und Ceration kommen. Die Behauptung, dass dies alles in Wahrheit eine einzige Operation sei, so dass „omnia simul et semel et eodem tempore et labore perpetrantur", erinnert wieder an die *Turba* (48).

Ein Zitat aus MORIENES „Nunquam fuit aliquid animatum ad nativitatem adductum, nec aliquid crescens, nisi per putrefactionem et mutationem" ist fast wörtlich in den *Interrogationes Regis Calid* enthalten (49). Von den drei Zitaten aus GEBER

(47) J. RUSKA, *Turba Philosophorum*, S. 127 und Übers. S. 200.
(48) Rede des BACSEM, Übers. S. 277.
(49) Vgl. *Artis Auriferae*, Bd. II, S. 27 u.

S. 272 und S. 275 habe ich bis jetzt bei flüchtiger Durchsicht noch keines aufgefunden.

Man sieht aus allen beigebrachten Belegen, dass der Verfasser die seit Anfang des 14. Jahrhunderts vorhandenen lateinischen Quellen zu seiner Arbeit benützt hat, selbst also — wenn die Ansetzung des HORTULANUS auf die Mitte des 14. Jahrhunderts richtig ist (50) frühestens gegen Ende des 14. Jahrhunderts geschrieben haben kann.

Was sonst noch AVICENNA von alchemistischen Traktaten zugeschrieben wird, ist so handgreifliche Fälschung oder so unbedeutend, dass ich mich darüber kurz fassen kann.

Über eine Einschaltung in RASIS bezw. ARISTOTELES *De Perfecto Magisterio*, die den Titel *Avicennae aquae rubeae ad tingendos IV spiritus sublimatos albos* trägt (51), habe ich in einem im Druck befindlichen Buch über RASIS und GEBER bereits das Nötige gesagt. Ein *Liber de XII aquis pretiosissimis maior omnibus*, von dem in D. W. SINGERS *Catalogue* S. 117 unter Nr. 127 eine Handschrift erwähnt wird, gehört jedenfalls in die gleiche Schriftengruppe. Eine Abhandlung des AVICENNA, die K. CHR. SCHMIEDER in seiner *Geschichte der Alchemie*, S. 98, unter dem Titel *Interpretatio epistolae Alexandri Magni* nach *Artis Auriferae*, Bd. 1, Nr. 16 anführt, verdankt lediglich einem Lesefehler ihren Ursprung. Im Inhaltsverzeichnis steht *Expositio Epistolae Alexandri Regis*, im Text S. 245 unter „*Mineralium Avicennae finis*" der ausführliche Titel „Cuiusdam Epistolae, quae Alexandri Macedonum Regis nomine circumfertur, interpretatio, abditam Philosophici Lapidis compositionem sapientibus acutissime declarans." Offenbar hat hier SCHMIEDER *Eiusdem* statt *Cuiusdam* gelesen und infolge dieser Flüchtigkeit den Traktat dem unmittelbar vorhergenannten AVICENNA zugeschrieben.

Meine Studie hat ihren Zweck erfüllt, wenn sie den Leser überzeugt hat, dass die pseudonyme alchemistische Literatur des Mittelalters auf ihre Herkunft geprüft und nach Entstehungszeit und Quellen näher bestimmt werden muss, wenn die Geschichte der Alchemie nicht für alle Zeiten ein Haufwerk von Legenden bleiben soll. Eine dieser Legenden ist die Behauptung, dass

(50) *Tabula Smaragdina*, S. 193.
(51) Vgl. Theatrum Chemicum, Bd. III, S. 107-109.

AVICENNA zahlreiche alchemistische Schriften verfasst habe. Wir haben gesehen, dass in den echten Schriften des AVICENNA keine Stütze für die Annahme zu finden ist, dass sich seine schroffe Ablehnung der Alchemie später in ihr Gegenteil gewandelt hätte. Das Buch *De Anima*, das umfangreichste und originellste ihm zugeschriebene Werk, ist rund hundert Jahre nach seinem Tode in Spanien entstanden und trägt alle Kennzeichen seiner späten Entstehung und seines kompilatorischen Charakters. Der reiche und vielseitige Inhalt würde eine Neuausgabe rechtfertigen, die selbstverständlich nur nach gründlicher Prüfung aller noch vorhandenen Handschriften in Angriff genommen werden dürfte. Andere AVICENNA-Schriften sind noch jünger und haben lateinische Alchemisten zu Verfassern, die sich mit mehr oder weniger Geschick der zu ihrer Zeit verfügbaren Literatur bedient haben.

(Berlin.) JULIUS RUSKA.

The Identity of the Manuscript entitled "Mr Nortons worke, *de lapide ph'orum*" with the Ordinall of Alchimy

Several works have been ascribed to THOMAS NORTON. According to BALE : " Nortonus composuit, *Alchimiae epitomen, Lib.* I. ... *De transmutatione metallorum, Lib.* I." (1). BORELL lists the " crede mihi " and " de transmut. metallorum " (2) as the works of THOMAS NORTON, and ASHMOLE (3), in his annotations to NORTON's *Ordinall of Alchimy* (4), which is generally regarded as identical with the *Epitomen Alchimiae* and the *Crede Mihi seu Ordinale*, states : " He wrote another *Booke De transmutatione Metallorum ;* and to these ... *Pitts* adds a third..." The reference in question (5) gives : " *De lapide Philosophico, Librum vnum. Epitomen Alchymiae, Librum vnum. ... De transmutatione metallorum, Librum vnum.* Et id genus alia multa." Since this time all three works have been listed as having been written by NORTON (6), although only the *Ordinall* has so far appeared in print.

We have not yet been able to ascertain whether the MS. *De transmutatione metallorum* is extant, but we have examined the

---

(1) BALE, *Scriptorum Illustrium maioris Brytanniae...* fol. 67, Basileae (1559), see also BALE, ... *Index Britanniae Scriptorum...* ed. POOLE and BATESON, p. 447. Oxford (1902).

(2) BORELL, *Bibliotheca Chimica...* p. 168, Parisiis (1654).

(3) ASHMOLE, *Theatrum Chemicum Britannicum...* p. 438, London (1652).

(4) The authorship of this work has already been called in question, see NIERENSTEIN and CHAPMAN, " Enquiry into the Authorship of the Ordinall of Alchimy," *Isis*, XVIII, 290 (1932); also HOLMYARD, " Thomas Norton and the ' Ordinall of Alchimy ' ," *Nature*, CXXXI, 520 (1933).

(5) PITS, ... *Relationum Historicarum de Rebus Anglicis...* i, p. 666, Parisiis (1619).

(6) JÖCHER, *Allgemeines Gelehrten-Lexicon...* iii, col. 982, Leipzig (1751); PORTER, *Dictionary of National Biography...* xli, p. 220, London (1895); FERGUSON, *Bibliotheca Chemica...* ii, p. 145, Glasgow (1906).

MS. entitled " Mr. Nortons worke, *de lapide ph'orum*." This MS. is listed by the Historical Manuscripts Commission as belonging to the Hatton Collection (7), and has been deposited by its present owner, the Earl of Winchilsea and Nottingham, with the Northamptonshire Record Society at Northampton. The examination of this MS. reveals that *de lapide ph'orum* is one and the same work as the *Ordinall of Alchimy*. There remains therefore only one more work of NORTON's so far not identified, namely, *De transmutatione metallorum*.

The following is a description of the MS. " Mr. Nortons worke, *de lapide ph'orum*," listed as F. H. 323, of the Finch-Hatton Collection, consisting of 68 folios made up as follows :

I) The first 125 pages are, with very slight differences, identical with the *Ordinall of Alchimy*, first published in English by ASHMOLE in 1652.

II) After three blank pages, two pages of verse, which are similar in subject-matter to the two pages of verse which come at the beginning of the *Ordinall* as published by ASHMOLE.

III) Six pages of additional notes, in a different handwriting, which may be provisionally described as a collection of *Philosophorum Dicta* in English (8).

To all intents and purposes the Finch-Hatton MS. is the same as ASHMOLE's version of the *Ordinall of Alchimy* (9). There are, however, the following differences : (i) The wording is slightly varied throughout. (ii) The spelling of the same words is rarely alike in the two versions, and the punctuation varies greatly. The MS. seldom begins the lines with a capital letter. (iii) There are no illustrations in the Finch-Hatton MS., but a description is given on pp. 8, 48-9 and 56-7 of the illustrations

(7) *First Report of The Royal Commission on Historical Manuscripts...* (Appendix) p. 34, col. 2, London (1874).

(8) See SINGER, *Catalogue of Latin and Vernacular Alchemical Manuscripts...* i, pp. 12-17, Brussels (1928).

(9) ASHMOLE, *Theatrum Chemicum Britannicum...* p. 455, London (1652), states : " In the *search* I have made after *Authentique Manuscripts* to compleate this *Worke*, a private *Gentleman* lent me a very faire one of *Norton's Ordinall*, which I chiefly followed; yet not admitting to compare it with fourteen other *Copies*. ... It had placed in the midle and bottome of the *Compartiments* of *Flowers*, *Birds* and *Beasts*, the *Nevell's* Coate of *Armes*, with others which that *Family* quartered. This induced me to believe it to be the *Originall* (or one exactly *Copied* from it) presented by the *Author* to *George Nevell...*"

which occur in the ASHMOLE version on pp. 12, 44 and 51. The Latin quotation from the small picture on p. 28 of the ASHMOLE edition is given on p. 29 of the MS., but four lines earlier, i.e., between the lines : " for lewde hope is fooles paradice..." and " The true taught Children made this confession..." The differences already remarked in wording and spelling are especially noticeable in the Latin quotations. At the end of chapter 5 ASHMOLE has a fifth illustration, which is not described in the Finch-Hatton MS., but in its place there are the following four lines in Latin (p. 108), which we have been unable to identify with the illustration :

" Terra cū in ignem. incident eius accumine dissoluta
fertur. resolluta in ignem aut aerem aut aquam
quousque partes ipsius congresse iterum sibique
Copulato terram reficiant / plato in (?)..."

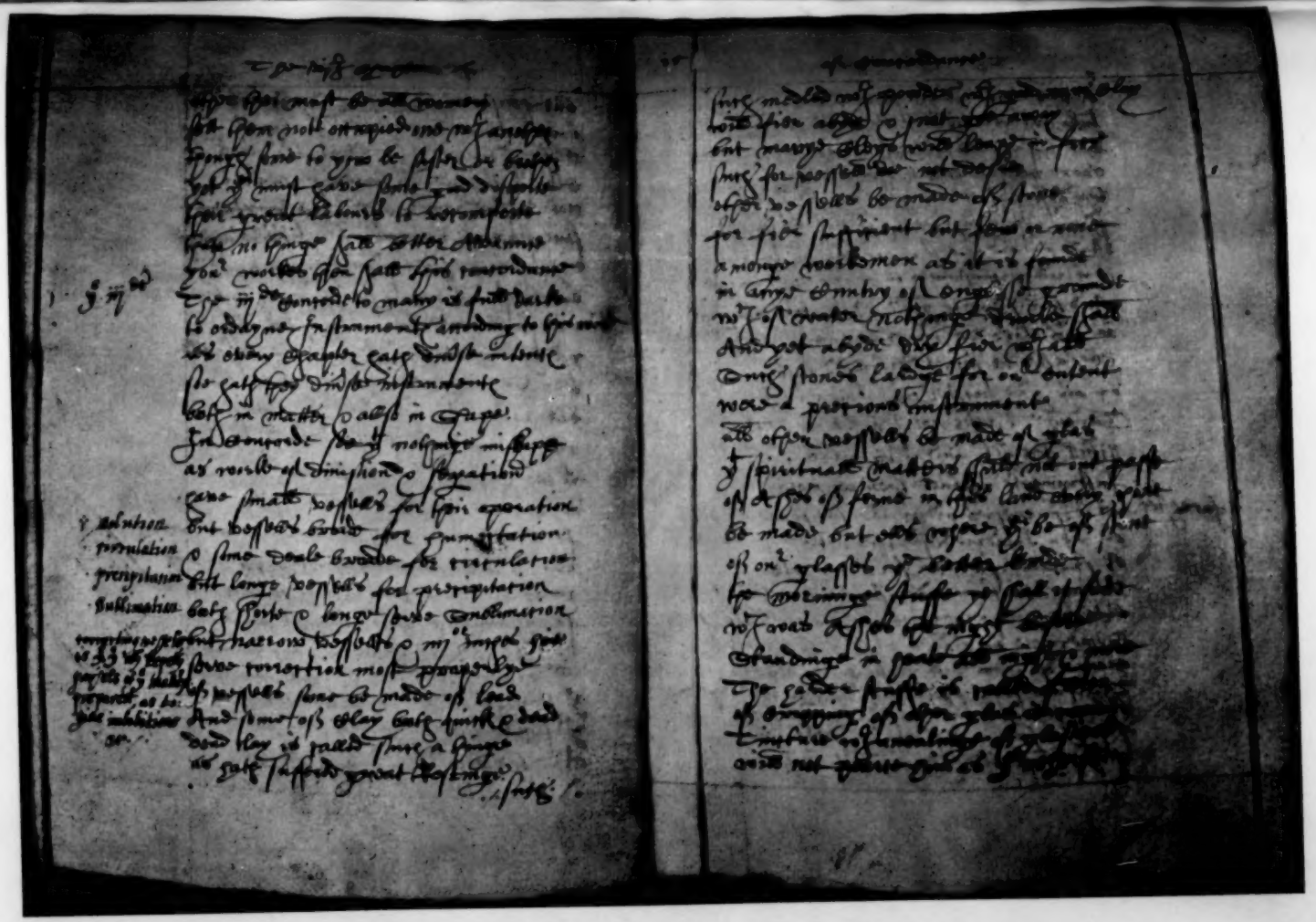
The last picture in ASHMOLE, p. 102, is not noted in the MS. (iv) In the MS. are a certain number of marginal notes, not in the published edition of the *Ordinall*. (v) Several lines are inadvertently omitted from the Finch-Hatton MS. :

1. (P. 4) ASHMOLE, p. 8 : " And him rebuked unrightfully..."
2. (Pp. 42-3) ASHMOLE, p. 39 : " Then made *Tonsile* to me his greate complainte,"
3. (P. 59) ASHMOLE, p. 53 : " Which helped *Kalid* at his neede,"
4. (P. 82) ASHMOLE, p. 72 : " While our *Stone* is in workinge;"
5. (P. 85) ASHMOLE, p. 74 : " And lesse Sower allso called Sapor Stiptick,"
6. (P. 90) ASHMOLE, p. 78 : " Fresher Liquor there is none in tast,"
7. (P. 107) ASHMOLE, p. 90 : " *Quod in ipsa albedine est rubedo occultata*,"
8. (P. 110) ASHMOLE, p. 93 : " Let such like Butterflies wander and Passe,"
9. The two lines in ASHMOLE (p. 97) :

" By me was found by Imagination,
Notably serving for Seperation..."

are telescoped into (p. 116) :

" by me was found for sep(ar)ation..."



2 Pages of the MS entitled "Mr. Nortons worke, *de lapide ph'orum* ".

M. NIERENSTEIN ; F. M. PRICE.

10. (P. 119) ASHMOLE, p. 100 : " The vertue infused resulteth of them all :"

Twenty lines which are omitted from page 78 of the MS. are inserted later, pages 103-104, with a note by the scribe to show where they should come.

The photographic reproduction from the Finch-Hatton MS., corresponding to pages 94, l. 23 to 96, l. 4 of ASHMOLE's edition, serves to show differences in the wording (i) and spelling (ii), whilst giving also an example of the marginal notes (iv).

Of particular interest are the two pages of verse which follow the three blank pages in the Finch-Hatton MS., as compared with those in ASHMOLE. MAIER (10) in 1618 prefaces his *Crede Mihi seu Ordinale* with 40 lines of Latin verse headed " Avthoris ipsivs." ASHMOLE repeats these verses and adds an English version of 46 lines. The verses in the Finch-Hatton MS., printed herewith for purposes of comparison, whilst equally well agreeing with the Latin, differ however from the English in ASHMOLE :

" This booke vnto Clerkes, declareth science grett
& herein lay men may, increase of Justice gett
A booke y^{t} helps allso, to honors plentifully
a booke it is y^{t} owe & nede, doth dryve from povertye
this booke of trust & trouth, is such a worthy thinge
y^{t} kinges therof take counsell may, & prelates gret lerninge
& profitable is to thosse, y^{t} blessed bynne
y^{t} doe desier for to live, in vertue out of sinne
A booke of secrets gret, w^{ch} god himselfe doth give
And is a path to Chosen men, y^{t} are of good beleue
Avaylinge to all such, y^{t} firme and Constant be
and owe be vnto them, that herein trust not me
The true and eke the false allso, doth seeke this alkemye
Y^{e} false wthouten number are, and they aye shalbe
of Covetous men, herew$^{\bar{th}}$ soe many shalbe enfecte
Y^{t} in a M' (thousand) thowsands, scant three shalbe elect
to him this noble science, full many one doth call
Y^{e} Noble, riche, & poore, the Just, & lerned all
Y^{t} yet no payne will take, nor noe dew tyme endure

(10) MAIER, *Tripus Aureus*... pp. 79-80, Francofurti (1618).

therfore to want y^e^ fruit therof, they may be suer
This booke vnto y^e^ Children, teacheth arte yenough
w^ch^ to p(er)ceave god giveth grace, and power to goe through.
This noble science y^e^^l^ only shall finde
Y^t^ loveth Justice w^th^ a blessed mynde
to false & Ravenous this science is forbyd
ffree giftes, for sinns, from vs is hidd.

" The kinges of England had ofte bin deckt w^th^ this
yf their hoope had bin stedfast in y^e^ lord of blis
but one theris shall come, & take hereof gret honor
w^ch^ Evills shall amend & change y^e^ old mannor
& when the same doth come, this Realme he shall Reforme
whos vertues & examples grett, to goodnes it shall turne
eternally y^e^ kinges, then people shall reioyce
w^th^ mutuall love to god alone, y^e^^l^ shall extoll y^e^^ir^ voyce
O thou kinge y^t^ this shall doe, pray thou y^e^ kinge of all
y^t^ he this Realme of ours p^r^seive, whos helpe is els but small.
for to y^t^ noble kinge that is of such good mynde
such noble thinges shall happ as he ne thought to finde."

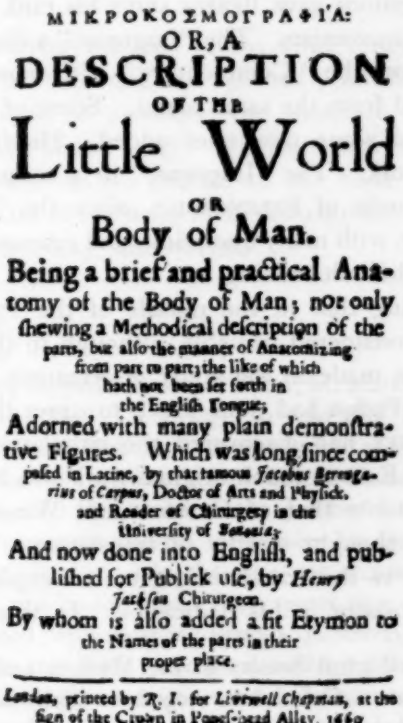
In conclusion we thank the Colston Research Society for financial assistance and Miss JOAN WAKE, Honorary Secretary of the Northamptonshire Record Society, for all the facilities granted us.

The University,
Bristol, England.

M. NIERENSTEIN.
FRANCES M. PRICE.

Jackson's English Translation of Berengarius of Carpi's "Isagogae Breves", 1660 and 1664 (*)

The enduring influence of the anatomical and surgical work of BERENGARIUS OF CARPI is shown by the translation into English of his "Isagogae breves," in 1660, one hundred and thirty-eight



ΜΙΚΡΟΚΟΣΜΟΓΡΑΦΙ'Α:
OR, A
DESCRIPTION
OF THE
Little World
OR
Body of Man.
Being a brief and practical Anatomy of the Body of Man; not only shewing a Methodical description of the parts, but also the manner of Anatomizing from part to part; the like of which hath not been set forth in the English Tongue;
Adorned with many plain demonstrative Figures. Which was long since compiled in Latine, by that famous *Jacobus Berengarius* of *Carpus*, Doctor of Arts and Physick, and Reader of Chirurgery in the University of *Bononia*;
And now done into English, and published for Publick use, by *Henry Jackson* Chirurgeon.
By whom is also added a fit Etymon to the Names of the parts in their proper place.

London, printed by *R. I.* for *Livewell Chapman*, at the sign of the Crown in Popes-head Alley. 1660

FIG. 1. Title-page of the 1660 Translation of the "Isagogae breves".

(*) From the Department of Medical History and Bibliography, University of California Medical School, and the Historical Section of the Lane Medical Library, San Francisco. Read before the California Medical History Seminar, December 12th, 1932.

years after its first publication, and also by BOERHAAVE's edition of " De Fractura Cranii," as late as 1715. The translation of the " Isagogae " was by HENRY JACKSON, a member of the Barber-Surgeons Company of London, and bore the title : *MIKPOKOΣ-MOΓPAΦIA* : or a Description of the Little World or Body of Man,... London,... 1660. A second edition with a slightly different title, appeared in 1664.

Besides the "Isagogae," first published in Bologna, 1522, BERENGARIUS had brought out an extensive "Commentary" on MUNDINUS in 1521. This work which contains a great many of his own original investigations gave BERENGARIUS his rank as the foremost of pre-Vesalian anatomists. The "Isagogae," a dissecting manual, is epitomized from the " Commentary," with most of the illustrations reproduced from the same plates. Some of the illustrations are omitted and some new ones added. He frequently refers to the larger work. The "Isagogae," in a sense, gives a better idea of the thought of BERENGARIUS, since the " Commentary " is over-scholarly, with many quotations and references to authority, which make it difficult reading.

It is interesting that in the middle of the 17th century this work was still considered valuable, although in the interval great strides had been made in the study of Anatomy. VESALIUS and his followers at Padua had done much to upset the old Galenical teachings, HARVEY had discovered the circulation of the blood, and throughout Europe much original work was being published; in England, notably that of GLISSON and WHARTON. Yet this little work was thought worthy of translation. WHARTON, who wrote a preface to the translation, gives an explanation for this, as does the translator in his dedication. In this JACKSON says :

" To The Worshipfull Society of the Mystery and Commonalty of Barber-Chirurgeons of London, together with all Students and Practitioners in Anatomy, Henry Jackson a Member of the aforesaid Society commendeth these his Labours.

(Most renowned Brethren, and Friends :) I Am provoked in my mind, after long deliberation, to publish this Work, being commanded in my first undertaking thereunto, by my aged Father, an ancient member of this Society, who having met with this Author in his Travels in Italy, esteemed it as a great Treasure, and too good indeed to be concealed ;

which being in old and curt Latine, cost me not a little pains to put it into smooth English ; and yet I never over-read the Work but I had comfort in it, and thought it worth all my pains...

By the help of which Book, I am of opinion, that the ingenuous Chirurgeon may be enabled, not only to Dissect from part to part, but also (where more excellent Physicians are not to bee had) to explain and read upon the parts, to the satisfaction of a Country Auditory, which effects hoping the Lord will crown this work withall, I take leave humbly to subscribe my self,

From my house in Southwark, A Lover of this Art, and of you all,
Febru. 25.
16 $\frac{59}{60}$ HENRY JACKSON.

THOMAS WHARTON was one of the most renowned anatomists in England of this time and in 1656 had published his " Adenographia," in which he discussed the structure and function of the various glands of the body. His name has been given to the submaxillary duct. In his preface to our book he gives a contemporary estimate of the work of BERENGARIUS, especially in relation to the intervening discoveries.

He says :

" To the Reader.
Courteous Reader,
I Am desired by my learned Friend and Tutor, M. Mark Franck, somtimes Fellow of Pembrook Hall, Cambridge, to read this Translation of his old acquaintance Master Jackson, Chirurgeon, and to write my thoughts, as an Epistle before it ; in obedience whereof I shall briefly address my self. I understand the Author to be Jacobus Carpus Bononiensis (because of his Figures, as also his mentioning his Commentaries upon Mundinus) printed in Latine in the year one thousand five hundred and thirty, about one hundred and thirty years agoe. Hee was in his time much esteemed for a most industrious, judicious, and expert Anatomist, and hath in this Book given good testimony thereof, for he hath in this Isagoge exceedingly much improved the administration of Anatomy, in many difficult parts of it, which is one of the principal qualifications of an Anatomist ; therefore its hoped this Book will bee as well worth the reading as any in that particular, by whosoever that will favour that ever Noble employment and exercise. Moreover, this good old Author is concise and short, without any tedious repetitions, and also writ in an excellent good order and method, and will neither spend time in reading, nor charge considerable. Its hoped the Reader will

easily bee persuaded to indulge this Writer with the common abatement necessarily granted to all our ancient Authors.

First, for that he writ so long before our for ever renowned Doctor Harvey, and therefore was not acquainted with those curious truths of the circuit of the bloud, which evidently demonstrates that the Veins reduce that bloud which was sent by the Arteries from the Heart abroad into the parts of the Body, and that the Heart with the Bloud and Spirits, is the chief Organ of vitality, the habitacle of the spirit of Life, common to us with Brutes : but the Brain, the Primum sensorium, the seat of the intellect, the complement of man, and the palace of the immortal soul.

The other excuse to bee entreated for the Author of this Work is also for his age ; for hee lived before our incomparable Doctor Glisson, had demonstrated the true uses of the Liver ; the exact way of Natures making Bloud ; the nature and course of the Lympha, and the motion of the Chyle ; and that the Splene poures no juyce, either sowre or sweet into the Stomach : which being supplied, our Author may happily pass compleatly current.

Formerly Italy bred many such learned Physicians and Philosophers as this Author, and then it was worth the while to journey to Padua to hear them, as other Nations anciently went into Aegypt : But now England by the industry of Harvey and Glisson, is the only Scene for both ; so that the politick Italian, if he will attempt the attaining to the knowledge of any thing considerable in either, must visit England, and ours stay to better purpose at home, unless the careful Father shall judge it necessary for the manning his Son, to hazard him such a Pilgrimage as to survey the ruines of old Rome, and Campus Martius the stately place of the new."

The justifiable national pride in the fame of English Anatomists had been heralded by HELKIAH CROOKE in 1615. He says, of the study of Anatomy in England at that time : " At this day we have also some worthy to be named, who if they listed, could turne the gaze of the worlde Westwarde, and time I hope will bring their monuments to light. In the meantime we have ventured to hold up this taper, at which if they please they may light their Torches."

Bibliographically, this copy of the 1660 edition, which is in the Lane Medical Library, San Francisco, is particularly interesting because, while the 1664 edition has been described, no mention has been found of this earlier one. There are copies of the 1664

edition in the British Museum and in the Surgeon-General's Library, but the work itself has never been adequately described, and some bibliographers, notably CHOULANT and FRANK (1), have denied the existence of such a translation. HALLER in his "Bibliotheca Anatomica," 1774-7 says of the editions of the "Isagogae,"—"English with the title a description of the body of man beeing a practical anatomy, Londo(n), 1664, 12°." GURLT (2), STIRLING (3), and PILCHER (4), also mention an English translation in 1664. HAESER in 1853 (5) referred to an English translation of the "Commentaria." CHOULANT who had never seen the English version says :

"... The mention of another edition of the COMMENTARY, Bonon. 1552, 4°, or folio, seems to be based on an error, just as the mention of an English translation of the COMMENTARY, London, 1664, 12°, is evidently due to a mistake."

and FRANK in a foot-note adds :

"Both Haller and Haeser quote this edition under the title *Μικροκοσμογραφία* ; or, A DESCRIPTION OF THE BODY OF MAN: BEING A PRACTICAL ANATOMY, London, 1664, 8°. Choulant's reasoning that this was based on an error is quite right, as the work contains illustrations from Vesalius with the text from Bauhin, Casserius, Paaw, and Laurentius."

The confusion is cleared up when it is realized that the work described by FRANK is the Anatomy of HELKIAH CROOKE, first printed by JAGGARD in 1615, and which, strangely, has the same short title as that of the 1664 edition of BERENGARIUS (compare figures 2 and 3). CROOKE's work is based on Vesalian anatomy and is a large folio of over a thousand pages. It was re-published in 1631, 1651.

---

(1) LUDWIG CHOULANT, History and Bibliography of Anatomic Illustration, translated and edited by MORTIMER FRANK. Chicago, 1920.

(2) E. GURLT, Geschichte der Chirurgie, Berlin, 1898, vol. I.

(3) WILLIAM STIRLING, Some Apostles of Physiology, London, 1902.

(4) L. S. PILCHER, A List of Books of Some of the Old Masters of Medicine and Surgery. Brooklyn, N. Y., 1918.

(5) H. HAESER, Lehrbuch der Geschichte der Medizin, Jena, 1853.

ΜΙΚΡΟΚΟΣΜΟΓΡΑΦΙΑ.
A
DESCRIPTION
of the Body of Man.
TOGETHER
WITH THE CONTROVERSIES
THERETO BELONGING.
Collected and Translated out of all the Best Authors of Anatomy, Especially
out of Gasper Bauhinus and Andreas Laurentius. By HELKIAH CROOKE, Doctor of
Physick, Physitian to His Majestie, and his Highnesse PROFESSOR in Anatomy and Chyrurgerie.
Published by the Kings Majesties especiall Direction and Warrant according to the first
integrity, as it was originally written by the AVTHOR.
Etiam Parnassia Laurus
Parva sub ingenti matris se subjicit umbra.
Printed by William Iaggard dwelling in Barbican, and are there to be sold, 1615.

FIG. 2. Title-page of HELKIAH CROOKE's "*ΜΙΚΡΟΚΟΣΜΟΓΡΑΦΙΑ*", 1615
(Courtesy of Dr. LEROY CRUMMER)

The only difference between the two editions of 1660 and 1664 is the title page. In the later version the title is shortened, and while both are printed for the same man, LIVEWELL CHAPMAN,

Bibl. Sloaniana
a 5431

ΜΙΚΡΟΚΟΣΜΟΓΡΑΦΙΑ:
OR, A
DESCRIPTION
OF THE
Body of Man:
BEING A
Practical Anatomy,
SHEVVING
The Manner of Anatomizing from Part to Part; The like hath not been set forth in the English Tongue.

Adorned with many demonstrative Figures

Long since Composed in Latine, by that Famous *J. Berengarius* of *Carpus*, Dr. & A. & P. Reader of Chirurgery in the University of *BONONIA*.

Done into English by *H. Jackson* Chirurgeon

By whom is also added a fit Etymon to the Names of the Parts, in their proper place.

London, Printed for *Livewell Chapman*, at his shop in *Exchange*. *All* ꝛim to ꝛ *hill*. 1664.

FIG. 3. Title-page of 1664 Translation of the "Isagogae breves"
(Courtesy of British Museum)

his place had changed from 'the sign of the Crown in Popes-head Alley' to 'his shop in Exchange Alley in Cornhill.' The edition of 1664 is evidently a re-issue.

The text is a literal translation of the "Isagogae" with few changes or additions. The illustrations are inserts and while some have been lost in the Lane copy, the descriptions in the text give an idea of those missing. Also some of the illustrations in the Surgeon-General's 1664 copy have been studied from photostats (6).

(6) Besides photostats from the Surgeon-General's Library and the British Museum, we have been exceedingly fortunate in having for comparison Dr. LEROY

Comparison of the illustrations in the translation with the originals show that they have not been improved, but are actually cruder than those of BERENGARIUS, many of which have artistic merit. An excellent example of slavish copying! (see Figures 4 and 5).

FIG. 4.
Illustration from "Commentaria," 1521.
(Courtesy of Dr. LEROY CRUMMER)

It is interesting to conjecture which of the earlier editions of the "Isagogae" was used by JACKSON in preparing his translation. Of these there are four which are well authenticated, Bologna, 1522, 1523, Strasbourg, 1530, and Venice, 1535. There is considerable difference between the two Bologna editions, as BERENGARIUS in the 1523 edition changed some of the figures,

---

CRUMMER's copies of the Commentary, 1521, of the 1530 edition of the Isagogae, and of HELKIAH CROOKE. We are most grateful to Dr. and MRS. CRUMMER for their kindness. We have also found the descriptions in various book-sellers catalogues most helpful.

notably those of the reproductive organs of women, and, among others, added three figures of the heart, one of the brain, and new drawings of the spinal column and the pelvis. These variations have been studied by ROTH in his "Andreas Vesalius Bruxellensis" where he also gives an excellent analysis of the

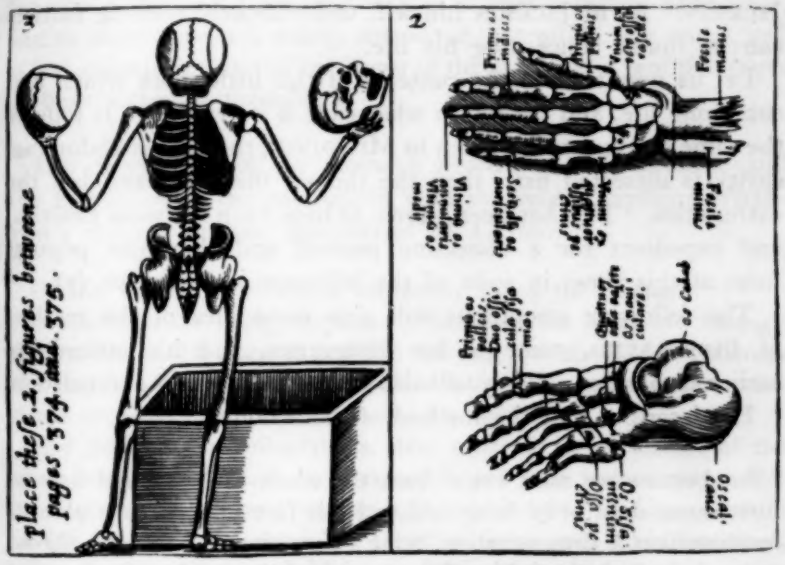


FIG. 5. Illustration from the 1660 Edition.

work of BERENGARIUS. An examination of the Strasbourg edition of 1530 shows that it has the same figures and arrangement as the 1523 edition, while that of Venice, 1535, is the same as the 1522 edition, with illustrations said to have been re-engraved. A study of the illustrations and their descriptions in the 1660 and 1664 translation reveals that they have the same number and arrangement as in the 1522 edition. For example, the figures of the heart and brain are not in. Thus this version probably is based on the 1522 edition or on the Venice edition of 1535. A further clue is found in the British Museum copy of the 1664 edition which has an illustrated frontispiece of a dissection scene. As the title on this has the same wording as in the 1660 edition it probably was present in the Lane copy, but has been removed. The dissection scene has been copied from that on the title page of the Venice 1535 edition. Some of the doctors have been adorned with pointed beards, in the English style. This dissection scene

had been used earlier on the title page of the " Opera " of HUGO SENENSIS, Venice, 1523. So in spite of the fact that WHARTON in his preface speaks of the original having been printed in 1530, apparently referring to the Strasbourg edition, the evidence points to the Venice edition of 1535 as the one possessed by HENRY JACKSON. As to JACKSON himself, unfortunately nothing further can be found concerning his life.

Let us now turn to the contents of this little work which had such long life, and try to see what gave it its vitality. It follows the same arrangement as seen in MUNDINUS, that is, the abdominal cavity is dissected first, then the thorax, the brain and last the extremities. This has been shown to have been the most practical and expedient for a dissecting manual and the most popular form at this time, in spite of the influence of VESALIUS. (7)

The following quotations will give some idea of the method of BERENGARIUS, some of his discoveries, and his attitude to earlier authorities. They will also show the style of the translation.

He describes first the method of dissection :

" But because we must begin from the whole as being best known ; First, some dead body being laid with his face upward, in a place fit for dissection & demonstration, being before washed, the hairs shaved, and very well cleansed from filth, even from the head to the feet ; we must know that the body is divided into four parts, that is, into three notable Bellies, and the extreams, to wit, the hands and the feet, with some others....

Of the Anatomy of the lower Belly.

The universal part being seen, I come to the particular, in which the Work-man must begin his incision from the lower belly, wherein there are many members first to be Anatomised and cast away, lest if they bee left behind, they should hinder the rest of the body by their putrefaction and evil savour.... "

Then follows the description of the contents of the lower belly. BERENGARIUS was the first to describe the vermiform

(7) SANFORD V. LARKEY, The Vesalian Compendium of GEMINUS and NICHOLAS UDALL's Translation. *Bibliographical Society. Transactions.* The Library, March, 1933.

appendix, which he evidently considered an abnormal occurrence. Speaking of the Intestine Saccum, or the caecum, he says :

"But this Intestine is often found frustrate in nature, because it doth none of the aforesaid things, and then it is also found fastned to the Intestine Colon and Ileon, and it is as it were a certain additament, and its shape appeareth strictly compacted, but within it is empty, and is less in breadth than the least finger of the hand, and it is of the length of three inches or thereabouts."

His description of the stomach illustrates his technique and the discussion of the connection with the spleen explains WHARTON's remark on the discoveries of GLISSON :

Of the Ventricule, which is commonly called the Stomach.
The Mesentereon being dispatched, blow up the Ventricle through the Duodenum left afore, as much as you can, that the chiefest greatness of it may bee seen, then you may reduce it to a mean inflation, that other things requisite in it may the better bee seen.

And first you shall observe its place which is in the middle of the whole body, the extream parts excepted, and it is immediately under the Septum transversum ; on the right side it hath the Liver, and on the left the Spleen, under it the Intestine Colon, and other Intestines, before the Omentum, and Abdomen, behinde the back, and the parts contained therein ; its situation is oblique, fastned to the back under the Diafragma ; its upper part is in the left side, that it might give way to the Liver, which is in the right, and placed on high : and that melancholy might the more easily go from the Spleen to the mouth of it : the lower part of it is in the right side, that it may give way to the Colon which is in the left taking up a great room ; but its lower part in the right side towards the Portanarium or gate, is less than in the left side toward the Colon, because in the right side the Liver taketh up a greater room than the Colon placed in the left ; also its lower part is in the right side, lest otherwise the Orifices should bee direct, both that the meat might bee the better retained, and that the Choler from the gall might the more easily enter into the Duodenum continued to the lower part of it."

Of particular surgical interest are his remarks on the removal of the uterus. Of the uterus he says :

"It may suffer every kind of disease ; it often falleth down, and it may all bee drawn forth out of the body, the health remaining. One Matrix

being corrupted, I saw drawn wholly forth by my Father in the Land of Carpus, which was cured, and lived long.

I also at Bononia, drew forth one other wholly, which was cancrenated in the year one thousand five hundred and seven, in the Moneth of May, which lived in health.

One other being corrupted, my Kins-man (by my brother) Danianius, drew wholly forth in my presence, in the Assembly of many Docters and Scholars, anno Domini one thousand five hundred and twenty, the fifth of October, that last by name, Gentilis, was the Wife of Christopher Briantus of Mediolanum, inhabitant at Bononia, in a Country called Lo Inferno, which at that hour one thousand five hundred twenty two, the tenth of November, was found, and exercising houshold affairs."

How closely he followed these cases is shown by a change in the 1523 edition, where he says that on the 10th of June, 1523, the patient was still in good health and exercising her household affairs.

The translation continues :

" If you seek greater things, look in my Commentaries upon the Anatomy of Mundinus, and there you shall have of the pregnant Matrix, and many other things. And these things are sufficient for the Anatomy of the Lower Belly."

Although he follows GALEN's view that there are pores through the septum dividing the ventricles of the heart, he is rather doubtful of the middle ventricle described by AVICENNA. MUNDINUS speaks of the third ventricle as in the wall between the other two ventricles, but says ' it is not one cavity but many small ones.' (8) BERENGARIUS says :

" It hath a three-fold Sinus or hollow place, or little house, or Ventricle; the right is bigger than the left, and the left cometh unto the extreamity of its point ; but the right is ended a little below that place.

Between them is a wall, gross and thick, called of Galen, Diafragma, in which are many small holes, going from the right Sinus into the left, being broader from the right than to the left ; those holes are dilated,

(8) Dr. SINGER's admirable annotated translation of MUNDINUS, in the 1493 Fasciculo di Medicina, Lier, Florence, 1925, has been of great help in comparing our text with that of MUNDINUS.

whilest that the Heart is abbreviated and opened, and they are shut up whilest it is lengthened and shut, by this means the blood being rarified and prepared, goeth from the right unto the left, where it is compleatly turned into the vital Spirit.

These Orifices are counted of Physicians for the middle Sinus. Galen, witness Avicen, calleth that Sinus, a ditch, and passage, and not a Ventricle, that it might be the Receptacle of the nutriment wherewith the Heart is nourished; which nutriment is thick and strong, like to the substance of it, and it is the mine of the Spirit, begotten in it of subtile blood; and it prevaileth, that the more temperate blood is in the middle Ventricle...

Nevertheless Avicen placeth a fourth part [of the Spirits] in the middle Ventricle, which he saith is temperate, but this is unknown to my eies; perhaps because in the middle wall of the Heart there pierceth blood, nourishing it; but it turneth into the substance of the thing nourished, because there (in my judgement) there is not blood without the Veins, unless in the right and left Ventricle."

One of the essential parts of GALEN's physiological system was the rete mirabile, or a network of blood-vessels, at the base of the brain, where the animal spirits were elaborated. He had observed this in calves, where it is well developed and by analogy assumed it in man. The independence of BERENGARIUS is seen in his refusal to accept this teaching. He says:

"And about that Net, some say, that there are two Glandules supporting it, and they say, that the helps of that Net are, that a subtilization of the vital spirit might be caused there, that being divided unto less branches it might bee the better altered, and the Animal spirit might be made; and perhaps that its little branches might bee more easily stopt, and might cause sleep, by the vapours raised up from the meat, and made thick by the Brain, falling down.

Nevertheless that Net I never saw, and I think Nature doth not work that by many things which it can doe by few; but Nature can make subtile these spirits in the least branches of the Arteries, descending above the Dura Mater cleaving to the bone Basilare, and ascending by the Pia Mater even unto the center of the Brain; therefore this Net is not given there between the Dura Mater, and the bone Basilare; many other reasons have I spoken upon this in my Commentaries upon Mundinus, to which for brevities sake I referre the Readers, and among other reasons, sensible experience is to me a guide."

The work closes with a description of the illustrations of the bones. (See Figure 5).

The first Figure of Bones.

In this Figure are seen the forms and situations, and also the true number of all the Bones of a mans body, except the bones of the head, and also the bones of the back ; all the junctures of which cannot bee seen, unless in bodies boyled, or dried in Church-yards..."

The third Figure of Bones.

You have in this Figure the number, shape, and situation of the Bones of the Hand and Foot ; in the Hand are the extremities of the two Fociles of the Arm, and eight Bones of the Rasetta, and four of the Pecten, and fifteen of the Fingers.

In the Foot you have the Os Calcanei, and the Os Cahab, and Os Naviculare, and four bones of the Rasetta, and five Bones of the Pecten, and fourteen Bones of the Toes.

These are those things which for the present wee have given to our Scholars for common Anatomy, for the end whereof let him be praised which is three and one, whom I most humbly entreat that hee may direct mee to greater matters. Amen.

And hee which is not content with these, let him have recourse to our most wholsome Commentaries upon Mundinus. Fare yee well in the Lord, yee which gather the Flowers of our Art after the manner of Bees ; for wee doe reject the outragious reader, the rest wee entreat and reverence : once more farewell."

His was truly the searching mind, 'gathering the flowers of our Art,' and we think we can say with THOMAS WHARTON, " this book will be as well worth the reading as any in that particular, by whosoever that will favour that ever Noble employment and exercise."

San Francisco.

SANFORD V. LARKEY.
LINDA TUM SUDEN.

Dr. Henry Power's poem on the Microscope

(Transcription of Sloane MS 1380, art. 16, kindly furnished by the authorities of the British Museum. Acknowledgment is hereby made to Capt. R. B. HASELDEN, curator of manuscripts, Henry E. Huntington Library, San Marino, California, for aid in the transcription. Punctuation and capitalization of the original have been adhered to as closely as was decipherable, but the long "ſ" has been modernized.)

TRANSCRIPTION

Microscopicall obseruations (1661)

In Comendation of ye Microscope.

Of all th' Inuentions none there is Surpasses
the Noble Florentine's Dioptrick=glasses.
For what a better, fitter, guift Could bee
in this world's Aged Luciosity.
To Helpe our Blindnesse so as to deuize
a paire of new & Artificiall eyes.
By whose augmenting power wee now see more
then all the world Has euer donn Before.
Thy Atomes (Braue Democritus [)] are now
made to appeare in bulk & figure too.
When Archimide by his Arithmatick,
numbred the sands, had hee But knowne this trick.
Hee might haue seene each corn a massy stone,
& counted them distinctly one by one.
Veiw[!] but the wonders in a Louse through whose
tralucent Body all Her entrayles shows.
As through a Chrystall watch I haue often seene
the Counter=motions of the wheeles within.
There yow may see the pulsing Heart driue on

the Blood in it's due Circulation.
If Natures skill in optick's yow would trye
then take a Flea & looke mee at her eye.
Where in an Emarauld Iris shee hath sett,
(empaled with white) a pupill all of jett.
Change but your object Looke at cõmon flyes
how theire soules peepe through Lattices of eyes.
In the gray=flye is liuely to bee Seene
an eye Dauncettad all of Black & greene.
As if ye nett=work of her eyes were made
of Irish=stick a greene with a dark shade.
In the wood=mite or =Louse yow may behold,
an eye of Trellice=Worke in Burnisht Gold.
Wich with a rare Contriuance Shee can moue
inwards or outwards as her objects proue.
Nay to the Spider prouidence hath giuen
plurality of eyes Some Six or Seauen.
Wich Like a Lockett of pure Diamonds Show
or like a Constellation in her Brow.
Here Argus eyes are fix'd & doe remaine
cleerer by farr then in the Peacock's Traine.
Nay lett mee Screw your Inquisition higher
looke but at mites & then you'le most admire.
How Nature could to Such a nothing Sett
both Heade, & Tayle, & Leggs, & mouth compleate.
Besides ye stomach, gutts, & Heart, & Spleene
wch lye within this Hollow mote unseene
To wch adde nerues, & Sinews & all these
cask'd in those Liueing exiguityes.
Oh for an otacousticon to Heare
the vocall Noyse of this throng[ed] Theater.
And sett of Animalls for who can tell
but they haue Seuerall cryes though inaudible.
Wee want thine eares Melampus that could Heare
when thou was cag'd up a close=Prisoner.
The Language that the Timber worme did speake
when they foretould thee y^{t} y^{e} Beame would Breake.
Looke into Liquors who would thinke that there
should Quick's breed in the keenest Viniger.

Wich though so uastly magnified appeare
noe bigger then ye smallest chire of Haire.
Here are the Curious Mathematick's Here
lyes ye rare skill of theire Artificeere.
All things hee made of nothing, but in this
hee made a thinge that lesse then Nothing is.
Who can the old Phylos[o]p[he]r renounce
that s[d] a thousand Soules might dance at once.
Vpon a needles point, when wee see Here
as many in one dropp of Viniger.
This to our mind the a'theriall wisdome bringes
how God is greatest in ye Least of things
And in the smallest print wee gather hence
the world may Best reade his omnipotence.
Goe on Dioptrick Artist's still goe on
may your glass=eyes euen Vie pfection.
With natures Noblest sense: from yow wee hope
such rare Emprouements of ye Microscope.
That ye Magnetick fluors of ye stone
shall bee as cleerely Seene as now there's none.
Nay then yow pretty sprit's & fairy Elues
that houer in ye aire Looke to your selues.
For with such prying Spectacles as these
wee shall see yow in yr owne essences.
Then shall I see a soule just when tis gone
as cleere as now I doe our will: & John.

Hen: Power D[r] of Physick.

NOTES

Line 1: This is also the caption of the first part of POWER's *Experimental Philosophy* (London, 1664); the other two parts are on mercurial and magnetical experiments respectively. The "Preface to the Ingenious Reader" is dated 1661 and was intended, from its content, to apply only to the first, or microscopical, portion of the work. Whether the poem was composed after the text of the *Experimental Philosophy* is not quite clear; it follows, in the MS, an article with the same caption, "Microscopical Observations, 1661" (see *Dict. Nat. Biog.*, art. on POWER). POWER's prose, it must be admitted, is far superior to his verse, but

many of the ideas and even phrases are similar in both, as noted below. Extensive quotations from the *Experimental Philosophy* (cited hereafter as *EP*) are not given here, for most of them have already appeared in the excerpts included in my paper on POWER and Sir THOMAS BROWNE (in *Isis*, 20, 344-66); the quotations have been modernized with regard to the long "ſ" and capitalization only.

4: Probably GALILEO was the noble Florentine; he described a microscope in his *Sidereus Nuncius* (Venice, 1610). FEDERIGO CESI, duke of Aquasparta, founder and president of the Accademia dei Lincei in Rome and a companion of GALILEO, together with his associate, JOHANNES FABER, physician to Pope URBAN VIII, made microscopical experiments in the early part of the 17th century; the latter's were published first, in his *Animalia Mexicana* (Rome, 1628). One observation from this work was paraphrased by Sir THOMAS BROWNE in his *Pseudodoxia Epidemica* (the "Vulgar Errors"; London, 1646: bk. 2, ch. 7, § 3), with which POWER, on account of his friendship with BROWNE, was no doubt acquainted. CESI's observations, published in his *Phytosophicarum Tabularum ex Frontispiciis Naturalis Theatri...Desumpta* (Rome, 1630), were mentioned by both FABER and BROWNE. (For an extended account see CHARLES SINGER: *Notes on the early history of microscopy* in *Proc. Roy. Soc. Med.*, v. 7 (1914), pt. 2, section of History of Medicine, p. 247 ff.).

"Dioptrical glasses (which are now wrought up to that height and curiosity we see) are but a modern invention: antiquity gives us not the least hint thereof, neither do their records furnish us with any thing that does antedate our late discoveries of the *telescope*, or *microscope*."—*EP*, $a3^r$.

"our modern engine (the *microscope*)"—*EP*, $b2^r$.

"*telescopes* and *microscopes*: which practical part of opticks is but yet in the rise."—*EP*, p. 78.

6-8: "Neither do I think that the aged world stands now in need of spectacles, more than it did in its primitive strength and lustre: for howsoever though the faculties of the ſoul of our primitive father *Adam* might be more quick & perspicacious in apprehension, than those of our lapsed selves; yet certainly the constitution of *Adam's* organs was not divers from ours, nor different from those of his fallen self, so that he could never discern those distant, or minute objects by natural vision, as we do by the artificial advantages of the *telescope* and *microscope*."—*EP*, $a4^r$.

"... how much therefore are we oblig'd to modern industry, that of late hath discover'd this advantageous artifice of glasses, and furnish'd our necessities with such artificial eys, that now neither the fineness

of the body, nor the smalness of the parts, nor the subtilty of its motion, can secure them from our discovery? "—*EP*, $c2^r$.

9-10 : " the minute bodies and smallest sort of creatures about us, which have been by them [the ancients] but sleightly and perfunctorily described, as being the disregarded pieces and huslement of the creation."—*EP*, $a3^v$.

11-12 : " ... [in " our modern engine (the *microscope*) "] we can see what the illustrious wits of the atomical and corpuscularian philosophers durst but imagine, even the very atoms and their reputed indivisibles and least realities of matter ..."—*EP*, $b2^r$.

" ... many more hints might be taken from the former observations, to make good the atomical hypothesis; which I am confident will receive from the *microscope* some further advantage and illustration, not onely as to its first universal matter, atoms; but also, as to the necessary attributes, or essential properties of them ..."—*EP*, p. 82.

14-16 : " Observat. XXXIII. *Corns of Sand, Sugar, and Salt.* It is worth an hour-glass of time to behold the crystal sands that measure it; for they all seem like fragments of crystal, or alum, perfectly tralucent... every corn about the bigness of a nuttmeg, or a walnutt ..."—*EP*, p. 42.

17-18 : " Observat. VI. *A Louse.* She appears the bignesse of a large crecket, the body diaphanous and transparent ... ... if she be laid on her back, you may perceive her body to be of escallop'd protuberances, diaphanous also, very handsome to behold"—*EP*, p. 9.

19-20 : " crystalline wheel-work of the heavens above us"—*EP*, $a3^v$.

21-22 : " In the centre of the middle spot [in the louse] there is a white film or bladder, which continually contracts and dilates its self upwards and downwards from the head towards the tayl; and alwayes after every pulse of this white particle or vesicle, then followes the pulse of the great dark bloody spot, in which, or over which, the vesicle seems to swim. This we observ'd two or three hours together, as long as the louse lived; and this motion of *systole* and *diastole* is most palpably seen, when the louse grows feeble and weak." —*EP*, p. 9.

24-26 : " Observat. I. *Of the Flea.* ... in it two fair eyes globular and prominent of the circumference of a spangle; in the midst of which you might (through the diaphanous cornea) see a round blackish spot, which is the pupil or apple of the eye, beset round with a greenish glistering circle, which is the iris, (as vibrissant and glorious as a cats eye) most admirable to behold."—*EP*, p. 1.

27-28 : " Observat. III. *The Common Fly.* ... But of all things her eyes are most remarkable, being exceeding large, ovally protuberant and most neatly dimpled with innumerable little cavities like a small grater or thimble ... The like foraminulous perforations or trelliced eyes

are in all flyes, more conspicuously in carnivorous or flesh-flyes, in the stercorary or yellow flyes that feed upon cow-dung : the like eyes I have also found in divers other insects, as the shepherd-flye or spinster-flye ... also in all sorts of scarabees ... also in all sorts of moth-flyes ... also in the may-fly, butter-flyes, scorpion-tail'd-fly, twinges, and earwigs; most clearly in the sloe-black eye of the crecket, and in the large eye of the dragon-fly or adderbolt."—*EP*, p. 4.

" Observat. II. *The Bee.* The eye of a bee is of a protuberant oval figure, black and all foraminulous, drill'd full of innumerable holes like a grater or thimble; and, which is more wonderful, we could plainly see, that the holes were all of a square figure like an honey-comb ... Now these holes were not absolute perforations, but onely dimples in their crustaceous *tunica cornea* ... for we cutt out the eye in a large humble-bee and crecket, and bared the shell or horney coat of the eye; and laying either the convex or concave side upwards (upon the object plate) I could easily perceive the little holes or dimples formerly mentioned."—*EP*, p. 3.

" Observat. XX. *The Glowworm or Glassworm.* Her eyes (which are two small black points or specks of jett) are pent-hous'd under the broad flat cap or plate which covers her head; which obscure situation, together with their exceeding exiguity, make them undiscernable to common spectators. Yet in the *microscope* they appear very fair, like black polish'd jett or marble, semi-globular, and all foraminulous, or full of small but very curious perforations (as in common flyes.) "—*EP*, p. 23.

" Observat. XXI. *Common Grasshoppers.* In those common grasshoppers ... there are some things remarkable. First, their eyes, which like other insects are foraminulous ... The like curious lattice-work I have also observ'd in the crustaceous *cornea* of the creckets eye ..."—*EP*, p. 24.

" ... we see the *tunica cornea* in most insects is full of perforations, as if it were a *tunica uvea* pinked full of holes, and whereas perfect animals, have but one aperture, these insects have a thousand pupils, and so see a hemisphere at once : and indeed 'tis worth our consideration to think, that since their eye is perfectly fixed, and can move no wayes; it was requisite to lattice that window, and supply the defect of its motion, with the multiplicity of its apertures, that so they might see at once what we can but do at several times, our eyes having the liberty and advantage to move every way (like balls in sockets) which theirs have not."—*EP*, p. 78.

29-32 : " Observat. IV. *The Gray, or Horse-Fly.* Her eye is an incomparable pleasant spectacle : 'tis of a semisphaeroidal figure; black and waved, or rather indented all over with a pure emerauld-green, so that it looks like green silk Irish-stitch, drawn upon a black ground,

and all latticed or chequered with dimples like common flyes, which makes the indentures look more pleasantly ..."—*EP*, p. 6.

"Dauncettad" = dancetté, an heraldic term meaning indented.

33-36 : "Observat. VII. *A Wood-Louse* or *Wood-Mite*. ... Her eyes are very protuberant, and globular, of a pure golden colour, most admirable to behold, especially when varnish'd with a full light, and most neatly latticed or mashed like a net (as hath been pre-observ'd in other insects.) And she seemed to have this peculiar artifice, that she can put out or draw in her eye at her pleasure; so that sometimes we could see them far more prominent then at others; and sometimes again the one eye more then the other : insomuch that in one of our critical observations, I could see more then a hemisphere of the eye at once ..."—*EP*, p. 10.

"Observat. XXIV. *The yellow Locust*. ... her eyes were very globular, protuberant, and large (as they are in all young animals) white, like two crystal beads, and most neatly lattic'd ... ... I sometimes see her eyes more protuberant than others, as if she could thrust them out, and draw them in at pleasure, as we have formerly observ'd in the wood-louse *observ*."—*EP*, p. 26.

37-40 : "Observat. VIII. *The House-Spider*. ... The first eminent thing we found in these house-spiders, were their eyes, which in some were four, in some six, and in some eight, according to the proportion of their bulk, and longity of their legs. These eyes are placed all in the forefront of their head (which is round, and without any neck) all diaphanous and transparent, like a locket of diamonds, or a sett of round crystal-beads ..."—*EP*, p. 11.

41 : "Neither wonder, why Providence should be so anomalous in this animal [the house-spider] more then in any other we know of (*Argus* his head being fix'd to *Arachne*'s shoulders.)"—*EP*, p. 12.

"... I could never find any animal that was monocular, nor any that had a multiplicity of eyes, except spiders, which indeed are so fair and palpable that they are clearly to be seen by any man that wants not his own. And though *Argus* has been held as prodigious a fiction as *Polypheme*,and a plurality of eyes in any creature, as great a piece of monstrosity, as onely a single one; yet our glasses have refuted this errour ..."—*EP*, p. 81.

43 : "... let us screw our enquiry a little further ..."—*EP*, p. 69.

44-49 : "Observat. XII. *Mites in Cheese*. ... What rare considerations might an ingenious speculator take up here, even from this singular experiment? of the strange and most prodigious skilfulness of Nature in the fabrick of so minute an animal (a thousand whereof do not weigh one single grain, (for one seed of tobacco is bigger than any of them)

and yet how many thousand parts of matter must go to make up this heterogeneous contexture? For, besides the parts inservient to nutrition, sensation, and motion, how small and thin must the liquours be that circulate through the pipes and vessels disseminated through those parts? nay, how incomprehensibly subtil must the animal-spirits be, that run to and fro in nerves included in such prodigiously little spindle-shank'd leggs?"—*EP*, p. 16.

"Observat. XVII. *The Mites or Lice found on Humble-Bees.* ... For since the minutest animal that comes within the reach of our *microscope*, is found to have a mouth, stomack, and gutts, for nutrition; and most, if not all, the *parenchymata* for circulation and separation of excrements, there can be no doubt, but they have also a continual perspiration and exudation through the habit of their body ..."—*EP*, p. 20.

"... it is not to tell in what a small particle of matter, life may actually consist, and exercise all the functions too, both of vegetation, sensation, and motion: so that, *Omnia sunt animarum plena*, may have more of truth in it, than he could either think or dream of that first pronounced it."—*EP*, p. 23.

50: "... the first tome of *zoography* is still wanting, the naturalists hitherto having onely described unto us the larger and more voluminous sort of animals, as bulls, bears, tygers, &c. whilst they have regardlesly pass'd by the insectile *automata*, (those living-exiguities) with only a bare mention of their names, whereas in these prety engines (by an incomparable *stenography* of Providence) are lodged all the perfections of the largest animals; they have the same organs of body, multiplicity of parts, variety of motions, diversity of figures, severality of functions with those of the largest size: and that which augments the miracle, is, that all these in so narrow a room neither interfere nor impede one another in their operations."—*EP*, *b2*v.

51: "I did try the use of the Otacousti[c]on, which was only a great glass bottle broke at the bottom, putting the neck to my eare; and there I did plainly hear the dashing of the oares of the boats in the Thames."—Pepys, *Diary*, 2 April 1668.

55-58: The myth of Melampus is related in *The Library* of Apollodorus the Athenian (bk. 1, ch. 9, § 12).

59-62: "Observat. XXX. *Of the little white Eels or Snigs, in Vineger or Aleger.* ... I have found them in all sorts of vineger, both in the keenest and smartest, as well as in the weakest and most watrish vineger ... ... you may ... see an infinite company of them swimming at the edges of the liquor, nay and in the body of it too, like so many shreds of the purest *Dutch* thread, as if the whole liquor was nothing else but a great shoal or mass of quick eels or hair-worms. ... as the liquor (dropt

upon your object-plate) spends and dries up, so you shall see those little quicks to draw nearer and nearer together, and grow feebler in their motion; and when all the vineger or aleger is dried away, then they lie all dead, twisted and complicated all together, like a knot of eels, and after a little time dry quite away to nothing. ... another remarkable thing, is, their exceeding exiguity; for certainly of all animals they are the least that can be seen by the bare eye, which is helped and advantaged also by the refraction of the water wherein they swim."—*EP*, p. 32.

63-64 : " ... in these narrow engines there is more curious mathematicks, and the architecture of these little fabricks more neatly set forth the wisdom of their Maker."—*EP*, $b3^{r}$.

" For the old dogmatists and notional speculators, that onely gaz'd at the visible effects and last resultances of things, understood no more of Nature, than a rude countrey-fellow does of the internal fabrick of a watch, that onely sees the index and horary circle, and perchance hears the clock and alarum strike in it : but he that will give a satisfactory account of those *phaenomena*, must be an artificer indeed, and one well skill'd in the wheel-work and internal contrivance of such anatomical engines."—*EP*, p. 193.

65-66 : " Observat. XXXVIII. *The small Dust, Powder, or Seeds of the lesser Moon-wort.* ... The exiguity and smalness whereof may very well be one of the *magnalia* of Nature, somewhat illustrating the great work of the Creation, and vast production from nothing."—*EP*, p. 46.

72-74 : " ... (alas !) those sons of sense [the ancients] were not able to see how curiously the minutest things of the world are wrought, and with what eminent signatures of Divine Providence they were inrich'd and embellish'd, without our dioptrical assistance."—*EP*, $a3^{v}$.

" ... we had need of all the advantages that art can give us, to discover the more mysterious works of that divine architectress [Nature]; but especially, when she draws her self into so narrow a shop, and works in the retiring room of so minute an animal [as the spider]."—*EP*, p. 82.

" ... but view [the butterfly's wings] ... in the *microscope*, and you may see the very streaks of the Coelestial pencil that drew them."—*EP*, p. 7.

75-86 : " And indeed, if the dioptricks further prevail, and that daring art could but perform, what the theorists in conical sections demonstrate, we might hope, ere long, to see the magnetical effluviums of the loadstone, the solary atoms of light (or *globuli aetherei* of the renowned *Des-Cartes*) the springy particles of air, the constant and tumultuary motion of the atoms of all fluid bodies, and those infinite,

insensible corpuscles (which daily produce those prodigious (though common) effects amongst us :) and though these hopes be vastly hyperbolical, yet who can tel how far mechanical industry may prevail; for the process of art is indefinite, and who can set a *non-ultra* to her endevours? I am sure, if we look backwards at what the dioptricks hath already perform'd, we cannot but conclude such prognosticks to be within the circle of possibilities, and perhaps not out of the reach of futurity to exhibit ..."—*EP*, $c2^v$.

" Observat. LI. *Of Aromatical, Electrical, and Magnetical Effluxions.* ... Indeed if our diopt[r]icks could attain to that curiosity as to grind us such glasses, as would present the effluviums of the magnet, we might hazard at last the discovery of spiritualities themselves ..."—*EP*, p. 57.

" Indeed, if our dioptricks could attain to that curiosity, as to grind us such glasses as would present the effluviums of the magnet; we might hope to discover all *Epicurus* his atoms, *Des-Cartes* his *globuli aztherii*, and all those insensible corpuscles which daily produce such considerable effects in the generation and corruption of bodies about us : nay, might not such *microscopes* hazard the discovery of the aerial *genii*, and present even spiritualities themselves to our view? "—*EP*, p. 155.

Berkeley, California. THOMAS COWLES.

The Beginnings of the Scientific Career of Joseph Priestley

In the standard biographies of JOSEPH PRIESTLEY occur a number of conflicting statements relating to the publication of his *History of Electricity* and to his election as a Fellow of the Royal Society. Thus we find in one (1) that the history was undertaken at the suggestion of Dr. FRANKLIN while in another (2) that FRANKLIN merely encouraged PRIESTLEY in the project. THORPE (3) and PRIESTLEY's latest biographer, ANNE HOLT (4) both agree that the publication of the *History* led to his election into the Royal Society, while PEACOCK and the *Dictionary of National Biography* assert that he was elected as a result of his experimental work in Electricity. Both events came very early in his scientific career and although the points themselves are of small importance, it would seem fitting, at a time when PRIESTLEY's admirers are celebrating the bicentenary of his birth (5) that the circumstances of the beginning of a career so distinguished should be recalled and to a certain extent restated from a fresh viewpoint.

This is to be found in the consideration of a number of letters mostly preserved in the Yates Collection of the CANTON Papers in the Library of the Royal Society. They throw an interesting light upon PRIESTLEY's relations with some of his Contemporaries and particularly upon the sequence of events leading to his election to the Fellowship of the Royal Society and to the publication of his "History and Present State of Electricity". In these events no one played a more important part than JOHN CANTON.

A glance at the CANTON papers as a whole reveals at once

(1) H. C. BOLTON, *Scientific Correspondence of Joseph Priestley*, 1892.
(2) *Dictionary of National Biography*.
(3) T. E. THORPE, *Joseph Priestley*. English Men of Science Series, p. 65.
(4) ANNE HOLT, *A Life of Joseph Priestley*, 1931.
(5) Born March 13th, 1733.

both the character and the work of the man to whom they were written. It is evident from the manner in which his numerous correspondents address him, that he was a man of wide learning and international repute. CANTON kept a School for boys in Spital Square and one gathers that often, after school hours, men—and women—of a philosophical turn of mind, went there to witness magnetic, electrical and other experiments. CANTON's phosphorus, the making of artificial magnets, his pith ball electrometer, the discovery of the chief facts of electrostatic induction and his experiments on the compressibility of water are among the notable contributions which CANTON made to the Science of his day. Such was his skill, that one of his biographers, Dr. THOMSON, describes him as "one of the most successful experimenters in the golden age of electricity".

He appears also to have been a gifted Mathematician, while a number of letters to and from the Astronomer Royal and others bear witness to his practical interest in observational astronomy. CANTON was evidently a charming personality as well as a distinguished Scholar, and it would seem that one of his greatest services to the Scientific progress of his time was the bringing together for discussion and experiment many who otherwise would have missed the delights of philosophical enquiry.

It is not surprising therefore, to find a man of PRIESTLEY's tastes and aspirations anxious to make the acquaintance of so distinguished a School Master.

It will be remembered that PRIESTLEY had himself founded a small School at Nantwich during his ministry there from 1758 to 1761. In his memoirs he says,

> " At Nantwich my school soon enabled me to purchase a few books, and some philosophical instruments, as a small air pump, an electrical machine etc. These I taught my scholars in the highest class to keep in order and make use of and by entertaining their parents and friends with experiments, in which the scholars were generally the operators, and sometimes the lecturers too, I considerably extended the reputation of my school; though I had no other object originally than gratifying my tastes. I had no leisure, however, to make any original experiments until many years after this time (6)."

(6) *Memoirs of Joseph Priestley*, reprinted from the edition of 1809 (1904), Section 65.

PRIESTLEY's success at Nantwich was rewarded by his appointment in September 1761 to the post of Tutor in Languages at the newly founded Warrington Academy. Although his own School "promised to be more gainful" and he would have preferred the office of teaching the Mathematics and Natural philosophy, PRIESTLEY accepted the new post because, as he puts it, he felt that his work at Warrington would be "more liberal and less painful" and "it was also a means of extending my connexions." This it certainly proved to be, for through the good offices of JOHN SEDDON, the virtual founder of the Warrington Academy, PRIESTLEY came to know CANTON and through him a large circle of friends in London. It is clear from SEDDON's letter of introduction, reproduced from the CANTON Collection in plate 7, that the writer had himself visited London, probably with the express purpose of seeing CANTON's School and of discussing with him details of organisation and curricula which CANTON was so well qualified to give.

As SEDDON points out, PRIESTLEY, now a young man of 32, has already attained considerable eminence as a man of letters. For his Chart of Biography (7), he had been awarded the degree of LL.D. of the University of Edinburgh, while his independent views on Education would be sure to commend him to a man like CANTON.

But by far the most important part of SEDDON's letter, in its far reaching consequences for the whole trend of PRIESTLEY's future career, is the postscript added overleaf, which can be seen showing through the paper near the top of the sheet. The sentence has been reproduced underneath by reversing the negative.

There can be no doubt that PRIESTLEY's introduction to FRANKLIN was brought about by JOHN CANTON, who for some years had

(7) The Chart of Biography, 3′ × 2′, represented an interval of time between 1200 B.C. and 1800 A.D. It was divided along its length into centuries and decades, the centuries being separated by lines drawn across the chart. It was divided along its whole length into six sections, in which occurred at the appropriate point of time, the name of the most eminent statesmen, Divines, Mathematicians etc., Poets, Critics, and Historians, the duration of each life being indicated by a line drawn underneath the name. It was thus possible to see at a glance the relation of one life to another and to see how many were contemporary at a given time.

been a firm friend of the American philosopher. It was CANTON who first verified in England, FRANKLIN's kite experiments on atmospheric electricity, and in this way was largely responsible for his election to the Royal Society. At the time of SEDDON's letter, FRANKLIN was in London as agent for Pennsylvania, urging privately among his many friends, and publicly before the House of Commons the repeal of the Stamp Duties levied on the American Colonies. Years afterwards, when the breach with the Motherland was an accomplished fact, FRANKLIN recalled with pleasure the opportunity he had had, even amidst the turmoil of political life in London, to attend the meetings of the Royal Society and to share with some of his friends the joys of philosophical discussion and experiment. From references in some of FRANKLIN's letters, it is clear that he was well acquainted with the CANTON family; he was doubtless a frequent visitor to Spital Square and it may well be that FRANKLIN and PRIESTLEY first met there,—or it may be that the introduction took place at the Royal Society rooms in Crane Court or at that well known meeting place of the intelligentsia, Batson's Coffee House " over against the Royal Exchange in Cornhill". To this last place, FRANKLIN often makes affectionate reference in his letters to English friends after his return to Philadelphia. There is no mention of the actual meeting, however, in FRANKLIN's memoirs, and PRIESTLEY's comments give no clue to its date or place.

" I was in this situation " (Warrington), he says, " when going to London and being introduced to Dr. PRICE, Mr. CANTON, Dr. WATSON (the physician), and Dr. FRANKLIN, I was led to attend to the subject of experimental philosophy more than I had done before (8)."

His son adds in a footnote :

" He always spent one month in every year in London, which was of great use to him. He saw and heard a great deal. He generally made additions to his library and his chemical apparatus. A new turn was frequently given to his ideas. New and useful acquaintances were formed and old ones confirmed."

The Rev. Dr. PRICE, to whom PRIESTLEY refers in the above

(8) *Priestley's Memoirs*, Section 80.

passage, was another contemporary and friend of both CANTON and FRANKLIN. The *Journal Book of the Royal Society* records that it was he who first introduced PRIESTLEY, during this Christmas visit, to that learned body. We read : Jan. 9th 1766, " The following gentlemen had leave to be present; Dr. PRIESTLEY and Mr. RADCLIFFE by Mr. PRICE."

Like PRIESTLEY, PRICE was a theologian by training and profession, but it was his publications on financial and political questions that made him famous. Of these, the most noteworthy are his " Observations on Reversionary Payments " (1771), " The Principle of the Doctrine of Life Annuities " (1783) and " An Appeal to the Public on the Subject of the National Debt." (1771). The first two practically laid the foundations of modern actuarial science and insurance practice, while the third in FRANKLIN's opinion was " the foremost production of human understanding that this century has afforded us." (9). Whether this estimate was extravagant or not, the book certainly had a profound influence upon English financial methods and largely influenced PITT in his establishment of the Sinking Fund for the reduction and ultimate extinction of the National Debt.

Politically, PRICE was an advanced Whig and as such was strong in his opposition to the war with the American Colonies. As FRANKLIN put it in a letter to PRICE when the war was at its height, (Feb. 6, 1780).

" Your great comfort and mine in this war is, that we honestly and faithfully did everything in our power to prevent it."

A pamphlet by PRICE on " Civil Liberty " is said to have contributed not a little to the Declaration of Independence and so strong were his sympathies known to be, that he was invited by Congress to take charge of American finance—an honour which he declined however.

Such a man as PRICE could not fail to attract the attention of the Opposition leaders in England. He became the close friend and adviser of Lord SHELBURNE—a fact of considerable importance in the story of PRIESTLEY's scientific career.

(9) In a letter to PRICE acknowledging a copy of the book, dated Craven Street, February 11th, 1772.

These three men, CANTON, FRANKLIN and PRICE, each one of outstanding ability in one or more branches of knowledge—in science, politics, finance, theology and education—these were destined to be PRIESTLEY's sponsors into a fuller and larger life and how eagerly he seized the opportunities these new friendships brought! This first visit to London on Christmas 1765 was unquestionably the turning point in PRIESTLEY's great career. It marked the change from School Master to Scientist and from preacher to political and religious reformer. All the latent powers of experimental investigation, the insatiable curiosity into physical and chemical phenomena, were awakened by what he saw and heard at Spital Square or Crane Court (10). Moreover, he was present at the House of Commons with EDMUND BURKE during FRANKLIN's examination on the Stamp Duties (11); (Feb. 3rd 1766); this and his private conversation with FRANKLIN and his friends explain much of what happened in later life. Henceforward his life became a curious blend; the man of science on the one hand; the champion of civil and religious liberty on the other.

PRIESTLEY was back in Warrington by February 14th (12), when he wrote to CANTON for the first time manifestly well pleased with his stay in London.

---

(10) The old home of the Royal Society.

(11) PRIESTLEY tells of this event in a letter dated Northumberland U.S.A. Nov. 10th. 1802, pubished in the Monthly Magazine, 15, 1-2, 1803.

" On the morning of the day on which the cause was to be heard, I met Mr. BURKE in Parliament-street, accompanied by Dr. DOUGLAS, afterwards Bishop of Carlisle; and after introducing us to each other, as men of letters, he asked me whither I was going; I said, I could tell him whither I wished to go. He then asking me where that was, I said to the Privy Council, but that I was afraid I could not get admission. He then desired me to go along with him. Accordingly I did; but when we got to the anti-room, we found it quite filled with persons as desirous of getting admission as ourselves. Seeing this, I said, we should never get through the crowd. He said ' Give me your arm,' and locking it fast in his, he soon made his way to the door of the Privy Council. I then said, ' Mr. BURKE you are an excellent leader;' he replied ' I wish other persons thought so too '."

(12) It is not certain when PRIESTLEY returned to Warrington. In the *History* (p. 581 1st Edition) he describes an experiment done ' sometime in the month of January ' though he does not say whether the experiment was carried out in Warrington or in London. If in Warrington, then PRIESTLEY must have travelled to London specially for the examination of FRANKLIN in the House of Commons. In view of the great uncertainty of his gaining admission, this seems very unlikely and the inference is that PRIESTLEY spent the whole of January in London, returning to Warrington some time after February 3rd.

" The time I had the happiness to spend with you " he writes, " appears upon review like a pleasing dream. I frequently enjoy it over again in recollection and ardently wish for a repetition of it. I wish in vain that it may ever be in my power to return in mind your generous communication of philosophical intelligence and discoveries."

In the latter part of the letter we have an interesting glimpse into what happened during the London visit. We read that PRIESTLEY had already made some of " CANTON's phosphorus " and ordered an electric kite with which he evidently intended to repeat or to extend FRANKLIN's and CANTON's investigations. Here too, we find the first reference to PRIESTLEY's plan for writing a Treatise on Electricity which he had discussed with FRANKLIN.

" I mentioned to Dr. FRANKLIN the plan of a treatise I am writing on Electricity. I have brought the history of it to the year 1742 when DESAGULIERS wrote (13). I am at a stand at present for want of material which I expect soon to receive. Nothing I ever wrote gave one so much pleasure, and I hope that so full an account that I shall give of the order and manner in which the discoveries in electricity have actually been made will not be unuseful to further discoveries. I have desired Mr. JOHNSON to apply to you for EPINUS. If you have any other books that will be of service to my design, I am sure you will not fail to communicate them."

It is quite evident from this passage and from his memoirs that the idea was PRIESTLEY's and not FRANKLIN's as is suggested in some biographies, though there is no doubt that FRANKLIN's encouragement and help in the provision of books and in the criticism of experiments greatly contributed to the success of the undertaking. The parallel account in the memoirs is as follows :

" I mentioned to Dr. FRANKLIN an idea that had occurred to me of writing the history of discoveries in electricity, which had been his favourite study. This I told him might be an useful work and I would willingly undertake it, provided I could be furnished with the books necessary for the purpose. This he readily undertook, and my friends assisting him in it, I set about the work, without having the least idea of doing anything more than writing a distinct and methodical account of all that had been done by others (14)."

(13) This introductory part occupies the first 69 of the 736 pages of the first edition.

(14) *Memoirs*, Section 80.

To return to the letter—probably the concluding paragraph and the postscript overleaf contain, from PRIESTLEY's point of view, its most significant message. Here he sets the wheels moving for his election to the Royal Society (see plate 8).

" My friends here imagine it would be a great advantage to the publication, if I were a fellow of the Royal Society, and have persuaded me to be a candidate for that honour. Dr. WATSON has been written to about it. I hope I need not formally ask your interest and that of Mr. PRICE. If Lord CH. CAVENDISH could be prevailed upon to join you, I should think the rest would be easy. "

In the postscript PRIESTLEY adds :

" If you think the few experiments I have made relating to Electricity would facilitate my admission to the Royal Society, I would send you a fuller account of them proper to be read before the Society, and for that reason I should (be glad) of a line from you or Mr. PRICE as soon as may be convenient."

This was written on Feb. 14th and we have seen that he was in the House of Commons on Feb. 3rd (15). It is quite evident that at this time PRIESTLEY was not relying on his electrical work to secure his election to the Royal Society. Indeed he only mentions the experiments as an after thought ! This is borne out too by the evidence of the memoirs.

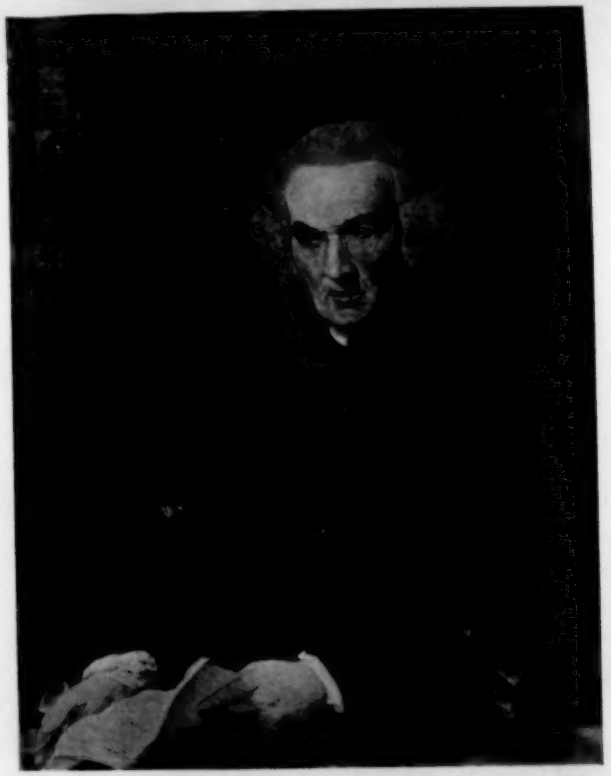
Referring to his work at Nantwich, PRIESTLEY states definitely that he had " no leisure to make any original experiments until many years after this time " (1758-61). Then, immediately following the passage in the memoirs quoted above (n. 14) we read.

" Having however, a pretty good machine, I was led, in the course of my writing the history, to endeavour to ascertain several facts which were disputed; and this led me by degrees into a large field of original experiments in which I spared no expense that I could possibly afford."

It seems certain, therefore, from the CANTON letter and from the above passages in PRIESTLEY's own memoirs that he did not

(15) See Note 11, page 86.

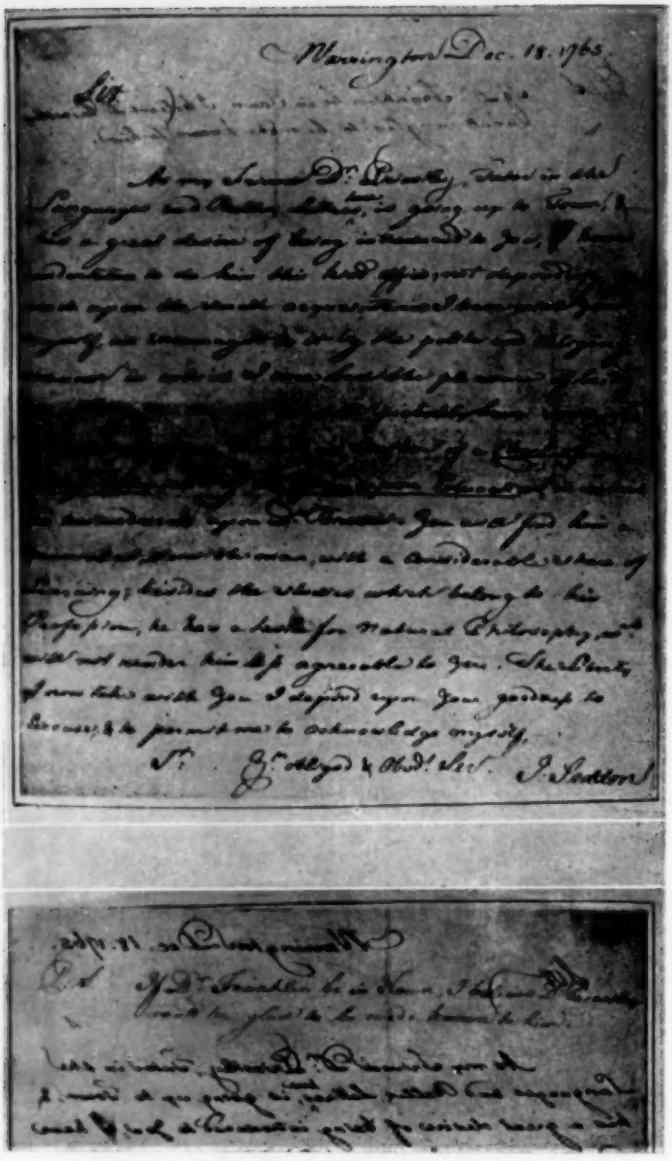
JOHN CANTON, F.R.S. (1718-72)



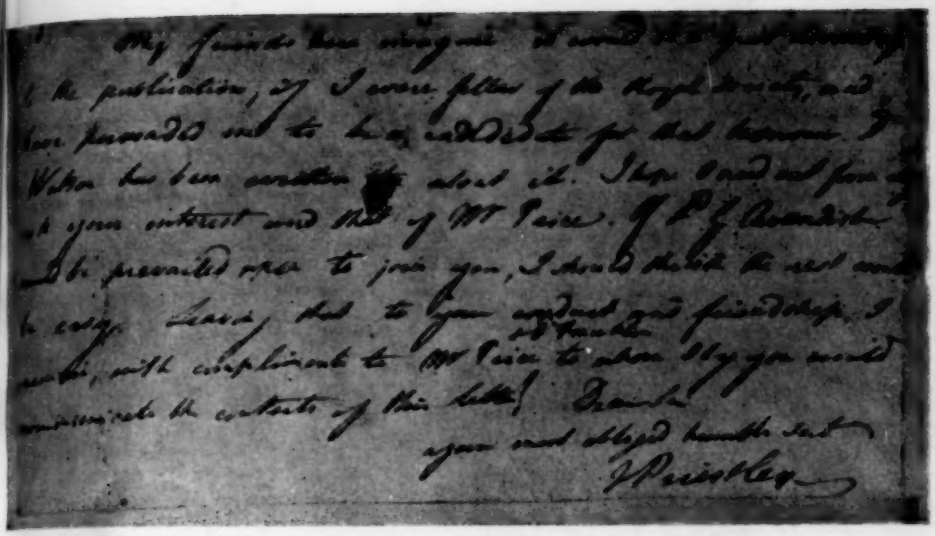
DR. PRICE

Isis, XXI, pl. 6.

W. C. WALKER.

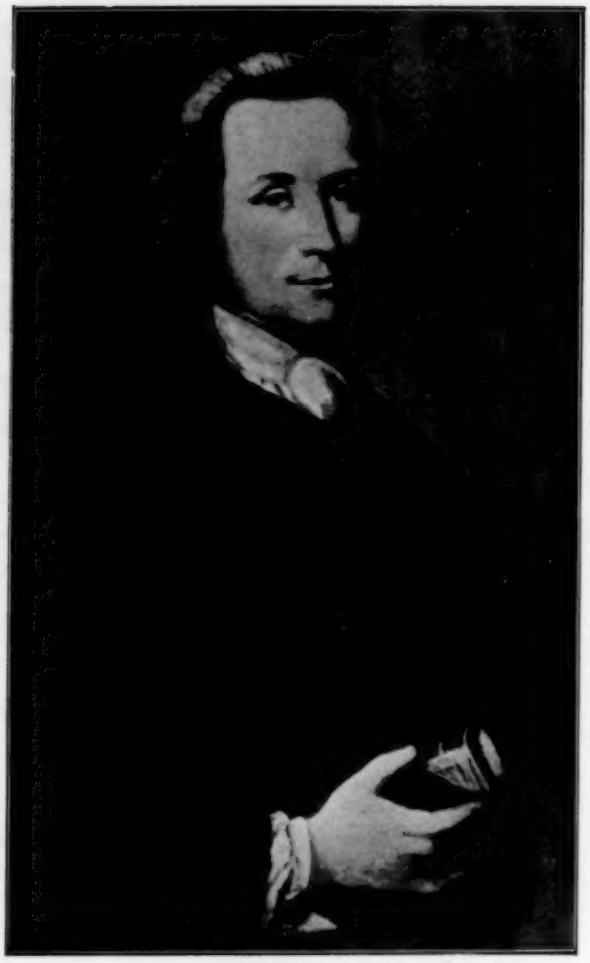


SEDDON's letter to JOHN CANTON



PRIESTLEY's autograph

B. FRANKLIN, from a print in the CANTON papers



JOSEPH PRIESTLEY (1733-1804)

Isis, XXI, pl. 9 W. C. WALKER

The above portrait, sometimes referred to as the " Leeds Portrait ", is the earliest known picture of PRIESTLEY. It represents him in the early thirties and is the only portrait of him as he appeared at the time of his election to the Fellowship of the Royal Society. The original is undated and its painter is unknown.

This plate is reproduced from a photographic copy of the painting made in 1860 at the suggestion of JAMES YATES F.R.S. and now preserved in the Yates Collection of Canton Papers in the Library of the Royal Society. The original is in a private collection of family portraits in the possession of a grand-daughter of Mrs. BILBROUGH (née ELLEN PRIESTLEY) now living in Cheshire. (See letter from the author on " The Leeds portrait " of JOSEPH PRIESTLEY.) *Nature*, June 17th 1933.

No 3320, vol. 131.

begin serious experimental work of an original character until after the London visit in Dec.-Feb. 1765-66 and it is equally clear that he could not have done much between Feb. 3rd and Feb. 14th. He was led *by degrees* into a large field of original experiments. Further, CANTON and PRICE either did not consider PRIESTLEY's work sufficiently important at the time or not necessary for the purpose of his election, for no paper was read to the Royal Society. No reply is preserved from CANTON or PRICE but passages in subsequent letters of PRIESTLEY seem to show that he repeatedly put off sending the account of the experiments until they were more complete. Thus to Price, Mar. 8th 1766.

"I shall write out a full account of the new experiments I have mentioned to Dr. FRANKLIN and Mr. CANTON, after a week or two, in which I shall have made other experiments which have a connection with them. In the meantime, my friends here think it will be best to mention them to some of the principal members, and to read the full account of them to the Society about the time of the election; but I beg of you, dear sir, and Mr. CANTON, not to have me proposed at all (if it be not done) unless you be morally certain it will be carried. I thankfully accept the offer of your name and Mr. CANTON's and desire you would act in concert with Dr. WATSON."

It is not surprising that the experimental work should have proceeded slowly, for in the meantime, PRIESTLEY was making amazing progress with the *History*. In the same letter of March 8th he says,

"You will by this time have received one number of my History of Electricity. I have another of them transcribed and the quantity of two more ready to transcribe."

He also mentions a letter written to FRANKLIN about a fortnight earlier, doubtless concerning some of his experiments.

A letter to CANTON, March 29 th, 1766, written from Warrington again refers to the progress of the *History*.

"You will herewith receive three of the last numbers of my History of Electricity. When I wrote to Dr. PRICE, I seemed to desire that they might be returned pretty soon, but I now beg you would take full time to peruse them, that you may give me your opinion of them at leisure and assist me to complete them."

Referring to the experiments again

"I do not know which of my experiments I can draw an account of to lay before the Royal Society. In the experiment with hot glass, I find, since I wrote to you, that I have been in some measure anticipated by Mr. KINNERSLEY, who could not charge a Florence flask filled with boiling water.

All my other experiments want pursuing and diversifying, before they can be fit to lay them before the public. I mention them to you, as to a friend, in whom I put confidence and who will point out any circumstances I may appear not to have attended to and thereby prevent my drawing hasty conclusions from them and imposing upon myself, or others (16)."

So far, PRIESTLEY's account of these early events in the memoirs agrees precisely with the evidence from the letters.

He says

"These experiments employed a great proportion of my leisure time; and yet before the complete expiration of the year in which I gave the plan of my work to Dr. FRANKLIN, I sent him a copy of it in print. In the same year five hours of every day were employed in lectures, public and private, and one two-month's vacation I spent chiefly at Bristol on a visit to my Father-in-Law (17)."

After answering criticisms that the *History* was too hastily composed, PRIESTLEY proceeds

"During the course of my electrical experiments in this year, I kept up a constant correspondence with Dr. FRANKLIN and the rest of my philosophical friends in London; and my letters circulated among them all, as also every part of my History as it was transcribed. This correspondence would have made a considerable volume and it took up much time; but it was of great use with regard to the accuracy of my experiments and the perfection of my work (18)."

The next paragraph in the memoirs, however, seems entirely misleading and is doubtless responsible for the inaccuracy of the statements in the PRIESTLEY biographies.

---

(16) Yates Collection of the Canton Papers. Library of the Royal Society.
(17) *Priestley's Memoirs*, Section 81.
(18) *Priestley's Memoirs*, Section 83.

" After the publication of my ' Chart of Biography ' Dr. PERCIVAL, of Manchester, then a student at Edinburgh, procured me the title of Doctor of Laws from that University; and not long after *my new experiments in electricity were the means of introducing me into the Royal Society*, with the recommendation of Dr. FRANKLIN, Dr. WATSON, Mr. CANTON, and Dr. PRICE (19)."

We have seen from the letters that PRIESTLEY's immediate object in attaining to the Fellowship was that " it would be a great advantage to the publication " of his *History* and that his electrical experiments were done during the writing of the *History* in order to clear up certain matters which were disputed, and that by degrees he was led into a large field of original experiments.

The account in the memoirs however, certainly gives the impression that " this large field of original experiments " was responsible for his election to the Royal Society, and one immediately associates with this his really important contributions to electrical knowledge, such as, the inverse square law in electrostatics, the conductivity of Charcoal and Black Lead, the explanation of PRIESTLEY's rings, the attempt to measure electrical resistance, a proposed method for measuring impedence, and the oscillatory nature of the Leyden jar discharge. Such a list of discoveries would indeed have been a worthy recommendation to the Society, but most, if not all this work was done after his election. It was certainly not sufficiently far advanced to merit publication; nor was it read to the Society.

It would seem that actually, PRIESTLEY's *literary* qualifications and not his electrical experiments were the grounds upon which he was proposed and elected. In WELDS' History of the Royal Society (20) are stated the rules governing the admission of Fellows at this time. The conditions are eight in number, any one of which was a sufficient qualification, but two of these would be applicable to PRIESTLEY, viz., 7. distinguished for literary or archaeological attainments; 8. as one who is attached to Science and anxious to promote its progress.

The notice in the Society's Journal Book and the certificate

(19) *Priestley's Memoirs*, Secion 84. The passage is not *underlined* in the Memoirs.

(20) WELD's *History of the Royal Society*, 1848. Vol. II, p. 578.

itself both bear out the same point of view. In the former, we read,

"Mar. 5th 1766. JOSEPH PRIESTLEY of Warrington LL. D., author of Divers works, particularly a Chart of Biography, was recommended for election and the certificate in his favour was ordered to be fixed up in the Publick Meeting Room. It was signed by W. WATSON, B. FRANKLIN, JOHN CANTON, SAM CHANDLER, and RICHARD PRICE."

It will be noted that the certificate was presented in a little more than a fortnight after the receipt of PRIESTLEY's first request to CANTON and before the letters to PRICE and CANTON of Mar. 8th. and 29th. quoted on p. 89, from which it is clear the electrical experiments were not ready for publication. If then, his new experiments were, as he says, responsible for his election, it could only have been through the action of his friends in London, who were acquainted with the earlier experiments in their imperfect form. Doubtless, his work, even at this stage, was sufficiently original to lead CANTON and his friends to believe that he would be a worthy member of the Society.

The terms of the Certificate are as follows :

"JOSEPH PRIESTLEY of Warrington, Doctor of Laws, Author of a Chart of Biography, and several other valuable works, a gentleman of great merit and learning, and very well versed in Mathematical and Philosophical enquiries, being desirous of offering himself as a candidate for election into this Society, is recommended by us on our personal knowledge, as highly deserving that honour; and we believe, that he will, if elected, be a useful and valuable member."

The certificate was exhibited for the usual three months and PRIESTLEY was elected on June 12th 1766.

Replying to the Secretary of the Society who had written to him announcing his success, PRIESTLEY says,

"I am now wholly engaged in electrical experiments; and I flatter myself that my enquiries will not appear to have been wholly without success." June 21. 1766 (21).

(21) RUTT's *Life of Priestley*. Vol. I, p. 66.

It is reasonable to suggest that only after four or five months work could PRIESTLEY have made any real progress with his new experiments, especially in view of his duties as a lecturer and the amazing speed with which he compiled the *History*—a large volume of over seven hundred pages. The experiments are described in the last part of the work in a section entitled " New experiments in electricity, made in the year 1766 " in which he presents the readers " with an account of such new experiments in electricity as this undertaking has led me to make."

The first edition of the *History* appeared in the early months of 1767 and in this the experiments were published for the first time.

Later letters in the CANTON Collection show that PRIESTLEY continued his electrical researches for some time after the publication of the *History*, and apparently by November 1767 he had drawn up an account to be read before the Royal Society. We gather this from a letter of FRANKLIN's written to CANTON dated Friday Nov. 27th (22). In this letter FRANKLIN informs his friend of the reception given to PRIESTLEY's paper by which it was hoped he would be awarded the Copley Medal. After considerable discussion, the paper was read to the Council—not to the Society—and FRANKLIN was asked to give his opinion on the merits of the experiments.

" Which I did on plain terms, declaring it as my judgement that the great pains and expense the doctor had been at in making them and the importance of the experiments themselves, well deserved that encouragement from the Society; and that it was a mark of distinction justly due to so much philosophical industry and sagacity. "

FRANKLIN continues,

" one that sat near me, told me that he was surprised at the account I had given him, as he had been assured the medal was intended to be bestowed on the doctor only for writing a History which was thought wrong, but it now appeared he had made many valuable new experiments etc.

---

(22) This letter, preserved in Vol. III, of the Canton Papers, is published in WELD's *History of the Royal Society* (Vol. I, pp. 67-69). The year is not given in the original, but WELD, by reference to other documents, considers it to be 1767.

Then a question arose, how far it was proper to give a medal for experiments that had not been sent to the Society till they were published and this occasioned a search for Sir Godfrey's will which could not be found..."

It was eventually decided to refer the matter to the next Council Meeting and in the meantime to search the records for the conditions of the award.

" how it will conclude at last " says FRANKLIN, " seems an uncertainty, for I think some persons are busy in an opposition to the measure. But I hope it will end in favouring merit, in which case, I think our friend cannot miss it."

Actually, as is well known, the Copley Medal was awarded to PRIESTLEY six years later—(Nov. 30th 1773) not for electrical work, but for his paper " On Different Kinds of Air " in which he described the impregnation of water with Fixed Air and advocated the use of the solution (" Soda-Water ") for the treatment of scurvy.

Apart therefore, from PRIESTLEY's own statement in the memoirs, the evidence seems to prove that he was not elected for his electrical work. Undoubtedly, CANTON, FRANKLIN and PRICE knew of his experiments and they saw in them promise of greater things, but that his work was not generally known to the members of the Society—not even to members of the Council—is obvious from FRANKLIN's letter to CANTON.

It is not, in any way, the writer's intention to accuse PRIESTLEY of deliberately falsifying the facts. Possibly he himself attached more importance to his earliest electrical experiments than his friends were likely to do and, in any case, it is not probable that these would have been accepted as a proof of any merit before they had been in progress many weeks. It would be quite easy for PRIESTLEY, writing his memoirs years after the events had occurred—after a life crowded with scientific, literary and political achievement—to have an unbalanced view of these early months of scientific research, and quite likely, he was under the impression that the experiments described in his letters to CANTON, PRICE and FRANKLIN were being freely circulated at the Royal Society.

It would appear then, that the true sequence of events may be reconstructed as follows :

During the Christmas visit of 1765, PRIESTLEY met CANTON, PRICE and FRANKLIN, with whom he discussed a plan which he already had for writing a *History of Electricity*. Moreover he saw at Spital Square some of CANTON's experiments, probably also in the presence of FRANKLIN and WATSON, and encouraged by their free communication of scientific knowledge and by their readiness to help with the *History*, he returned to Warrington full of zeal for the new self-imposed task. Then it occurred to him, or to his friends, that a fellowship of the Royal Society would assist the sale of the book and accordingly he solicited the good offices of his newly made acquaintances in London. From the promptness with which they acted, there can be no doubt that PRIESTLEY had already made a favourable impression; his literary work and his interest in philosophical experiments, were to them doubtless sufficient recommendation for his election to the Society and it seems certain that these were the grounds upon which he was elected; quite apart from any experimental work he may have started at the time. Indeed, by reference to the " New Experiments " in the *History* itself, it will be found that although some are recorded as having been done in February, March, and April of 1766, these are relatively few and unimportant compared with those which followed between June 1766 and January 1767, and all the work was first published in February 1767, eight months after his election to the Fellowship.

It seems therefore that neither the *History* nor the experiments were responsible for PRIESTLEY's admission to the Royal Society, but that the election, sought primarily to assist the publication of the *History*, was secured for literary ability, and the electrical experiments arose at first incidentally to verify or disprove certain facts in the historical narrative. The new experiments which grew out of these, while sufficiently important to merit the award of a Fellowship or even as FRANKLIN thought, of the Copley Medal, were done, for the most part, *after* PRIESTLEY's admission to the Society. In more modern times, solid achievement must precede election to that most coveted of all scientific distinctions; in PRIESTLEY's case the election came first, results afterwards.

How far PRIESTLEY justified the confidence of his friends is

well known. The precise reasons for his election, in spite of the opinion given above, may still be a matter for conjecture, and in any case, they do not affect the splendid contributions which he was able to make to the progress of his generation. But the influence of that Christmas holiday of 1765, in moulding his future activities, is unmistakable: it seems to follow him through the years as one by one, he embarked upon some new venture.

In the first place, the *History* at once brought PRIESTLEY an international reputation and made him an authoritative referee on all matters electrical. In response to an invitation in the Preface, foreigners, and his own countrymen alike, freely communicated their discoveries to him, while he, in turn, passed on to the Royal Society anything worthy of notice and incorporated the new facts in subsequent editions of the *History*. Thus, to mention a few, VOLTA's account of the electrophorus came first to PRIESTLEY and early papers by HENLY and BENNET appeared in the *Philosophicol Transactions* as " Communicated by Dr. PRIESTLEY."

The real merit of his electrical work has already been mentioned : indeed, it has been said that these researches were by far the most logical of all his scientific work. Certainly, they were a fitting prelude to his better known chemical investigations.

These too, it is interesting to observe, first began in connection with his electrical work at Warrington. In the *History* and in the CANTON letters, he refers to experiments with " Mephitic Air," that is " Air which has passed through a charcoal fire or through the lungs of animals " in which he sought to show that Mephitic Air was a conductor of electricity as distinct from common air which is an insulator. The experiments gave ambiguous results but they led him finally to discover the conductivity of charcoal which he thought contained " Mephitic Air in its fixed state " (May 4th, 1766). It is easy to see how his removal to Leeds (September 1767), where he found a convenient supply of Mephitic Air at hand in a neighbouring brewery, stimulated fresh enquiry into this substance and led ultimately to important and far-reaching developments in pneumatic chemistry. The shift in his interests from electricity to chemistry is fully reflected in the CANTON letters.

To follow him still further, it is well known that the period

of his greatest productivity as a chemist, was from 1772 to 1780, when he was Librarian to Lord SHELBURNE, the leader of the Liberal group in the Whig Party. It was during these years of comparative leisure that he discovered oxygen and through his meeting with LAVOISIER that a new impetus and a new trend were given to chemical thought. The significant point is, that it was his friend RICHARD PRICE who recommended PRIESTLEY to Lord SHELBURNE and that it was FRANKLIN to whom he wrote for advice when faced with the invitation to leave his ministerial work for the new post (23).

Finally it was his controversy with BURKE which brought to a climax his unpopularity in England and caused him to seek refuge among FRANKLIN's countrymen in America.

The men who first gave to PRIESTLEY their hospitality and their goodwill during the Christmas season and who sponsored his election to the Royal Society, were indeed his truest friends. His triumph was theirs also and no one recognized his obligation more than PRIESTLEY.

" Without them " he says, " neither my experiments nor this work would have had any existence (24)."

A statement which is true, not only of the *History*, but of the whole of his scientific labours.

(London.) W. CAMERON WALKER.

---

(23) The writer bases this statement upon the contents of a letter from FRANKLIN to PRIESTLEY dated London, Sept. 19th 1772, in which he says :

" In the affair of so much importance to you, wherin you ask my advice, I cannot for want of sufficient premises advise you *what* to determine, but if you please I will tell you *how*."

FRANKLIN then outlines a method he always adopted under similar circumstances of judging between pros and cons, in which he cancels out reasons of equal weight on either side until he arrives at the balance—a process which he terms " Moral or Prudential Algebra." P. 522, vol IV, FRANKLIN *Works :* BIGELOW.

PRIESTLEY's appointment as Librarian dates from December 1772.

(24) From the Preface to " History and Present State of Electricity."

The plates are reproduced with the kind permission of the Council of the Royal Society. The author also desires to express his sincere thanks to Mr. H. W. ROBINSON for the facilities he has given in obtaining other data from the Library of the Royal Society.

Plant Pathology in the Penultimate Century (*)

This review of plant pathology in the eighteenth and early nineteenth centuries is confined to this country and those Western European countries from which the United States of 1833 chiefly derived its people and its culture. There is, however, no indication that the account would be greatly changed if a review were made of the knowledge of plant diseases found in oriental authors or writers of classic or medieval Europe, since there is little evidence that this early learning had penetrated into the agricultural information and practice of the century now under consideration.

The close association of Mycology and Plant Pathology since the days of the TULASNE Brothers, de BARY, and M. J. BERKELEY leads to the too frequent impression that plant pathology developed solely as a branch of mycology. Actually, it had a considerable independent history prior to the middle of the last century, in which mycology played little part. This is clearly shown, for example, by the books on plant pathology published during the period.

TEXT BOOKS OF PLANT PATHOLOGY

What seems to have been the most widely circulated text during the period was JOSEPH JACOB PLENCK's (20) *Physiologia and Pathologia Plantarum* published in Vienna in 1794. The original was in Latin but there were subsequently published two German editions, one French and one Italian. In addition to the Latin text I have examined the French edition published in Paris in

(*) An address at the opening of the meeting of the American Phytopathological Society in connection with the celebration of the " Century of Progress," Chicago, 1933.

1802 through the courtesy of the Lloyd Library at Cincinnati, and through the courtesy of an indulgent wife a manuscript English translation.

The portion devoted to diseases is divided into eight sections of which the two largest, each occupying about 20 per cent of the space devoted to plant diseases, are Class I—External Injuries and Class V—Decays.

Under External Injuries are classified wounds due to different types of instruments, gnawing and rubbing of beasts of burden, lightning, hail storms, winds, snow, and for their treatment we find frequently recommended the use of FORSYTH's composition, regarding which more will be said later. Here are discussed also frost cracks and defoliation, including that due to laboratory fumes, and various types of "ulceration" including that due to insects. Under Decays are placed injuries due to certain boring insects, the rusts of cereals to which is assigned just 15 lines, and the smuts of cereals occupying twice as much. In all, the discussion of these two classes of diseases takes up about two pages or a total of 289 words. Ergot occupies more space than the rusts and smuts together, obviously because it is a cause of human disease.

Evidently diseases due to fungi played a relatively small part in the plant pathology of a century ago. As in all texts of that time much attention is devoted to classification of the diseases, to which are given names either identical with or derived from those of human diseases. We find such distinctively medical terms as ulcer, hemorrhage, gangrene, even cancer and leprosy, although this last proves on further examination to be nothing more deadly than an incrustation of the trunk and branches with various species of mosses and lichens.

When one becomes accustomed to the strange terminology and the lack of illustrations and really studies the text much appears that is worthy of commendation, perhaps even emulation. It is in a degree refreshing to find an author with the courage and breadth of view to discuss and to include under plant pathology injuries due to hail and snow, wounds due to insects and mammals, the antagonism of certain green plants, defoliation due to poisonous fumes, the effect of excess water on the soil, sun scald, frost injury, including its prevention by smudging and the use

of water, floral sterility, somatic mutation (although not under that name), and the effect of an excess of nitrogenous fertilizer on the growth and fructification of crops, to which attention has recently been directed in discussions of the significance of the carbohydrate-nitrogen ratio.

The descriptions of the diseases are excellent and their arrangement such that diseases producing similar symptoms are actually classified together where a grower would be likely to look for them. For example, under spotting we find sun scald, spots due to insects, rust spots and smut spots. Wherever a control method is known it is given briefly following the description of the disease. In general these control methods are those which have been worked out empirically, but in the treatment of wounds much reliance is placed on FORSYTH's composition. No doubt PLENCK places too much confidence in this but we must remember that it had an extraordinary amount of publicity and distinguished sponsorship not very long before the publication of this text.

In sharp contrast to the brief descriptions and practical advice given by PLENCK is the *De Morbis Plantarum* published in 1773 by J. B. ZALLINGER (32). The first part of this volume is a highly philosophical discussion of disease and health in plants with much theoretical matter as to the means by which health and disease can be recognized and the significance of the composition, structure, and motion of plants. This text and several other contemporary ones are briefly reviewed by WHETZEL (30).

In LOUDON's *Encyclopedia of Agriculture* (16) which was apparently one of the leading reference books of its kind during the period, we find an entire section devoted to " vegetable pathology or the diseases and casualities of vegetable life." Here the diseases are somewhat elaborately and carefully classified. A wide range of abnormalities is included and the relative amount of attention given to the various classes does not differ greatly from that noted in PLENCK's book.

Naturally enough the most detailed descriptions of diseases or of experiments on their control are not found either in general text or reference books but in books dealing with diseases of special crops, and in papers before various learned and Horticultural Societies. From these sources the information regarding plant pathology of this century is largely drawn.

Timber treatment

Since the eighteenth century was an era of wooden ships it is not surprising that governmental and other agencies gave attention to means of preserving timber. As early as 1730, the Dutch Government is reported (3) to have investigated the possibilities of Corrosive Sublimate for use in timber preservation. Copper Sulphate was advocated for use in this connection by De Boissieu and Bordenave in 1769, and both this salt and Zinc Chloride were recommended by Thomas Wade as early as 1815. A method of applying chemicals in the preservation of timber by means of vacuum and pressure was employed by M. Breant, in France, in 1831.

The coming of the railway with the necessity of timber for ties gave, of course, a greatly added stimulus to interest in wood preservation, especially in England where the forests could not produce anything like a sufficient quantity, and in September 1832, John Howard Kyan was granted a patent for the use of corrosive sublimate as a timber preservative, this is No. 6253, and the specifications call for 1 lb. of the salt to 5 gallons of water, used hot or cold. The first British patent for the use of creosote or related products was, however, not granted until 1838.

Tree surgery

Among tree surgeons the most widely known during the century under discussion was William Forsyth (9) whose work had so great an influence in calling attention to plant diseases and their importance, that the circumstances of his rise to international fame will be given in detail : About 1785 or 1786 the attention of the British government was directed toward the diminishing quantity and quality of British timber available for use in the Royal Navy, the supply from this side of the Atlantic having been cut off by the recently achieved independence of certain American colonies.

Mr. Nichol, of Redbridge, Hants, Purveyor for Portsmouth Dock, at that time, is authority for the statement that the " average of the damaged timber brought to that place was never less than

one-fourth of the total quantity of timber brought in annually; and not unfrequently it amounted to a third." Commissioners were duly appointed by parliament to examine into the state of the woods, forests and land revenues of the crown, and their anxiety to prevent the loss or further damage of much valuable timber led them to WILLIAM FORSYTH, Gardener to His Majesty at Kensington and St. James, who apparently already had some reputation as a tree surgeon. After some correspondence, a committee of the House of Parliament duly examined the results of Mr. FORSYTH's work, and the King granted a reward to Mr. FORSYTH for disclosing the method of making and using the "composition" which soon became so widely known. It is given below:

"Take one bushel of fresh cow dung, half a bushel of lime rubbish of old buildings (that from the ceiling of rooms is preferable), half a bushel of wood ashes, and a sixteenth part of a bushel of pit or river sand: the three last articles are to be sifted fine before they are mixed; then work them well together with a spade, and afterwards with a wooden beater, until the stuff is very smooth, like fine plaster used for the ceilings of rooms."

The formula has been the cause of so much amusement in later years that insufficient attention has been given to FORSYTH's directions for tree surgery and the application of his composition. This seems to be a case, by no means without parallel in the history of medicine, in which cures were actually caused by careful manipulation while the credit went to some simple drug.

It may be recalled that at the time of FORSYTH's activities, it was a common custom, in cutting branches from a tree to leave a stub two or more feet in length attached to the trunk, on the theory that the trunk would thus be less liable to injury. FORSYTH, on the other hand combats this idea and points out that the "remaining stem of the branch so cut away, dying soon after, becomes a ready conduit for conveying pernicious moisture and disease to that part of the tree with which it is connected; and so on, in time, to the whole."

His directions for tree surgery follow:

"The composition being thus made, care must be taken to prepare

the tree properly for its application by cutting away all the dead, decayed, and injured part, till you come to the fresh, sound wood, leaving the surface of the wood very smooth, and rounding off the edges of the bark with a draw-knife, or other instrument, perfectly smooth, which must be particularly attended to ; then lay on the plaster about one-eighth of an inch thick, all over the part where the wood or bark had been so cut away, finishing off the edges as thin as possible : then take a quantity of dry powder of wood-ashes mixed with a sixth part of the same quantity of the ashes of burnt bones ; put it into a tin box, with holes in the top, and shake the powder on the surface of the plaster, till the whole is covered over with it, letting it remain for half an hour, to absorb the moisture ; then apply more powder, rubbing it on gently with the hand, and repeating the application of the powder till the whole plaster becomes a dry smooth surface."

To the above directions he adds the following which may well prove of assistance in making a smooth callus (p. 324) : " When the new wood, advancing from both sides of the wound, has almost met, cut off the bark from both the edges, that the solid wood may join, which, if properly managed, it will do, leaving only a slight seam in the bark."

Withal WILLIAM FORSYTH was somewhat of a salesman. In the activities of modern tree surgeons with all the advantages of news print and radio, I have seen no advertising comparable with that received by FORSYTH in the report to the Lords Commissioners of his Majesty's Treasury, by a committee of lords and gentlemen headed by the Marquis of Abercorn, the address of the House of Commons to his Majesty, and the ensuing royal grant for disclosing his formula. Nor has it been my fortune to see a matter presented in a more suave and tactful manner than his in a letter addressed to the Honourable the Commissioners of the Land Revenue, dated April 24, 1789, from the Royal Gardens, Kensington, in which he modestly admits that his remedy is known only to himself, but properly insists that as a servant of his Majesty he does not think himself at liberty to form any engagement that must inevitably call him for a time from his Majesty's service in his Royal Gardens at Kensington, but should his Majesty be graciously pleased to think his services would be productive of a national good, and will condescend to permit him to be absent, he will " with the greatest pleasure and alacrity "

engage in the undertaking. He further asks permission to lay before the Honourable Board several specimens of parts of trees which have been helped by his method, and asks them to visit his Majesty's Royal Garden at Kensington. In a short memorandum dated April 28, 1789, he points out that the entire cost of treatment including the labors of the men for the operation will not exceed sixpence per tree, and thoughtfully adds: "I should also observe, that in this expence I have not put down any thing for myself, leaving that wholly and altogether to your further consideration."

Harvesting and handling fruits and vegetables

The attention paid in the United States to methods of harvesting and handling in relation to the decay of fruits and vegetables gives special point to opinions on these matters held over a century ago.

William Forsyth's *Treatise on the Culture and Management of Fruit-Trees*, 1802, has a chapter on this general subject. He notes specifically that "As apples shaken or beaten down with a pole never keep in Winter, they ought all to be hand-picked by a person standing on steps made on purpose." He then goes on to state that they should be placed in baskets in the bottoms of which some dry grass mowings have been placed, and that the harvester should pick the fruit carefully, and lay it gently into the basket... for if it be in the least bruised it will not keep. For the same reason, great care must be taken in emptying the fruit out of the hand-baskets, when full, into the large baskets or hampers. "If more than one large basket be wheeled at once, the lower ones must not be so full as to let the bottom of the upper one touch the fruit." Fruit should be gathered if possible in dry weather. He cautions those who erect new shelves in their fruitrooms, to have the timber well seasoned, and to use white deal (that is pine) in preference to red (fir).

For shipment to any considerable distance, Forsyth recommends a box two feet long by fourteen inches broad and fourteen inches deep (very nearly the size of the present apple box of the Northwestern United States) made of pine, well secured with iron clamps at each corner. The fruit should be wrapped individually in soft paper or in some cases, first in vine leaves

and then in paper. The more perishable fruits, currants or cherries, were packed in layers of dry moss. He concludes with this remark : (p. 240). " My reason for being so particular on packing of fruit is, that I have known instances of its being totally spoiled in the carriage from improper packing.

" By pursuing the above method we have never failed of success; and if fruit be packed according to the foregoing directions, it may be sent to the farthest part of the kingdom, by coaches or waggons, with perfect safety."

FORSYTH's text was by no means unique in including the subject of harvesting and handling. In a letter to the Caledonian Horticultural Society in 1814 the Rev. ANTHONY DOW says " The Horticultural Society, I observe, most judiciously limit not their attention to the mere culture of plants; but extend it also to the preservation of their produce." Mr. DOW then describes a method for keeping potatoes through the summer by putting them in a pit which has been watered and shaded.

In the same volume of the Memoirs of this Society, Mr. JAMES STEWART describes a method of keeping apples and pears by putting them in a layer of sand, and Mr. ROBERT INGRAM of Torry discussed at some length the proper conditions for ripening and keeping pears. He discussed the proper construction of fruit rooms and the desired temperature, and he points out that the house should be so made as to " admit plenty of free air when required, as this will be necessary for keeping the fruit in good condition." He also insists that the drawers in which the fruit is kept should be made of hard-wood for he has " known fir spoil flavor of the fruit."

Gas storage had at least been considered in this period for in 1821, M. BÉRARD reported (4) that most fruits can be preserved for a certain period by picking a few days before they are ripe and storing in an atmosphere free from oxygen. Peaches, plums, and apricots were held in good condition for 20 to 30 days, and pears and apples for 3 months; but when these periods were exceeded, the fruit failed to ripen upon removal to normal air. This pioneer work was apparently not put to practical use and was largely lost from even the scientific world. It was not until over 70 years later that further studies along this line were seriously pursued.

Diseases due to nematodes

The disease of wheat caused by *Tylenchus tritici* was first noted in England by Needham (19), who published in 1744 his observations on *Worms Discovered in Smutty Corn*. Needham, however, in spite of the fact that he recognized the diseased condition and described the nematodes in the diseased parts did not recognize their causal relation. The real significance of the accompanying nematodes was pointed out by Roffredi (25) in 1775 and 1776.

More important from the standpoint of Plant Pathology is Schwerz' discussion originally published in 1825 (26), of the disease of Clover now known to be caused by *Tylenchus dipsaci* even though he did not recognize its parasitic nature. He pointed out that the disease usually called "*Stock*" affects Rye, Oats, and Buckwheat, as well as Clover, but not Wheat or Flax. He describes the symptoms of the disease, notes its spread from small infected areas in the field, and that such spread may be prevented by ditching. He further emphasizes the fact that soils may become so generally infected as to be regarded as "*toll*" or as we would say in 1933 "sick." His remedies for this condition are clean fallow or growing flax, peas, or potatoes.

My friend and colleague, Doctor Steiner, is authority for the statement that while some of the crops then regarded as free from the disease are now known to be susceptible the methods thus recognized in 1825, that is, clean fallow and crop rotation are still the only practical means of controlling this disease under field conditions.

Bacterial diseases

As is well known bacteria were not recognized as a cause of disease in plants until 1878. It should, however, be noted that in the first manual of orcharding published in the United States, that is 1817, Mr. William Coxe (6) of Burlington, New Jersey, mentions in his preliminary statement regarding pears, "a species of blight which is sometimes called fire blight." His description which includes the fact that it causes "a morbid matter to exude from the pores of the bark, of a black ferruginous appearance,"

can leave no doubt that he is dealing with the disease that we now know to be due to *Bacillus amylovorus*. Further he observes that this blight is very serious on St. Germain variety but less serious on Seckle. He had seen limbs and even large branches of the Seckle variety destroyed, but out of 50 trees he had never lost one, whereas he has already lost more than 50 trees of the St. Germain variety. He notes that the disease occurs in the most open airy situations, and in every kind of soil, and that further the disease spreads more rapidly in the trees which grow luxuriantly.

In passing attention may be called to the fact that in his proposed revision of this book, the manuscript (23) for which is now in the Library of the United States Department of Agriculture, but which was never published owing to the death of Mr. COXE in 1831, there occurred descriptions or illustrations of the following diseases :

Leaf Blight	Apple Scab	Fruit Spot
Pear Scab	Flyspeck of Apple	Peach Scab
	Bitter Rot	

It is evident that in the second edition Coxe intended to devote much more attention to plant diseases.

Another bacterial disease recognized over a century ago was the Corky Scab of potatoes. It was apparently first mentioned in 1825 in LOUDON's (16) *Encyclopedia of Agriculture*. " Scab, that is to say, the ulceration of the surface of the tubers has never been explained in a satisfactory manner. Some attribute it to the ammonia from the dung of the horse, others to alkali, and certain others to the use of wood ashes on the soil. Not using diseased seed and planting in other soil are the only known means of preventing the malady."

DISEASES DUE TO MYXOMYCETES

Club Root of Crucifers

The symptoms of club root or as it was often called " Anbury " are so conspicuous on cabbage and turnips that the disease could

hardly escape the attention of the gardeners although the cause was not determined. Club Root was the subject of some concern in Great Britain about 1780, and certain important facts were observed. It was known that club root was more likely to be serious on ground where turnips were grown but that the disease could be present in soil which had not borne this crop for MARSHALL (18) reports finding the disease on turnips and says, " the piece from which I drew these specimens was an old orchard, and never before bore turnips in the memory of man." More significant is the fact that the control of the diseases by the use of lime applied either as " marl " or " dry hydro-sulphate of lime " was apparently well established by field tests before the end of the century we are discussing.

Club root was so serious on turnips in Scotland, where the disease was called " fingers and toes," that as a result of a discussion at a meeting of the Caledonian Horticultural Society held in September 1819, the Secretary sent out requests to various gentlemen for information regarding the disease.

The reply of the Right Honourable Sir JOHN SINCLAIR published in the Memoirs of the Caledonian Horticultural Society, Vol. 4, p. 550, contains an interesting modification of the lime treatment in which lime together with soot, soap and a little tobacco and salt were mixed in a tub of water and the solution poured from a watering pot around the roots of each plant. This contribution is further notable because he points out the similarity of this disease to one common on " the cabbage tribe of plants " called " The Club."

FUNGOUS DISEASES

To attempt to list all the fungous diseases recognized prior to 1833 would be both tedious and futile. Mention will be made of only a few diseases well recognized at that time and for which distinctive control methods had been worked out.

Stem Rust of Wheat and Barberry Eradication

Nothing can be more chastening to the pride of professional plant pathologists than the story of how through a period of at

least a century the practical grain growers of western Europe and eastern North America persisted in the belief that barberries did injure wheat and in their eradication even in the face of the opposition, argument and ridicule of the mycological and agricultural students of the day. The barberry bush, probably a native of the mountains in central and western Asia, was taken to Italy and Spain by the Arabs in the seventh century. During the seventeenth century it was introduced into northern Europe and North America, and became popular for its fruit and as a hedge plant. Soon thereafter injury to cereals growing near the barberry plants began to attract attention. The firmness with which the conviction that the barberry bushes were injurious to their wheat came to be held by farmers is attested by the laws passed in an effort to combat the barberry, first in northern France in 1660 (1), in certain New England States between the years 1726 and 1766, and in several European countries shortly after 1800.

Since no one could give a really satisfactory explanation of the injurious effect of the barberry on the wheat, and as all mycologists and many others insisted that the idea was preposterous, a hard fight amounting in some sections to a bitter feud was carried on by those who thought the bush was injuring the crop and those who were defending it. A very great degree of success in eradicating the barberry must have been attained at certain periods, and in certain localities.

MARSHALL (17) writing in 1787 testifies both to the controversy and to the scarcity of barberry bushes in Norfolk as follows :

" It has long been considered as one of the first of vulgar errors among husbandmen, that the barberryplant has a pernicious quality (or rather a mysterious power) of blighting the wheat which grows near it.

" Being desirous of ascertaining the fact, be it what it may, I have enquired further among intelligent farmers concerning this subject.—They are, to a man, decided in their opinion as to the fact ; which appears to have been so long established in the minds of principal farmers, *that it is now difficult to ascertain it from observations* ; *barberry plants have* (*of late years more particularly*) *been extirpated from farm-hedges with the utmost care and assiduity*." [italics mine].

MARSHALL further goes on to give the results of setting a small

barberry bush in a large piece of wheat. He noted that the affected area of wheat " resembled the tail of a comet, the bush itself representing the nucleus; on one side of which the sensible effect reached about twelve yards; but on the other, not more than two yards; the tail pointing towards the south-west : so that probably the effect took place during a north-east wind.

" At harvest, the ears near the bush stood erect, handling soft and chaffy; the grains slender, shrivelled and light.—As the distance from the bush increased, the effect was less discernible, until it vanished imperceptibly."

It remained, however, for THOMAS ANDREW KNIGHT to make in 1804 (22) what appear to be the first experiments in the inoculation of wheat from barberry. He sowed a row of wheat round a barberry plant in his garden, and several small portions of seed of the same kind in similar soil at a considerable distance from it. He examined the two lots in July, and this keen observer reports as follows :

" Examining the barberry bush attentively, I found upon its fruit a species of fungus similar in color to that on the straws of the wheat; but its seed vessels were larger and more spherical. I was, however, much disposed to believe the parasitical plants of the same species, and that the difference in the form and size of the seed vessels arose only from the difference of the nutriment they derived from the wheat, and from the acrid juice of the barberry. The plants of wheat which grew at a distance from the barberry bush, remaining free from the disease, I carried a branch of barberry, with diseased fruit upon it, to one of them, and wetting it with water, I brushed the wheat plants with it, repeating this operation three successive days. I at the same time applied a part of the diseased straws which had grown near the bush, to other plants of wheat, which were free from disease, leaving upon them so large a portion of the seeds and seed vessels of the mildew, as to be visible without the aid of a lens. In the course of ten days the plants of wheat, which I had endeavoured to infect by means of the barberry branches and fruit, became covered with disease, whilst those to which I had applied the mildewed straws were not sensibly affected. I attributed the health of these to the want of moisture necessary to make the seeds of the mildew vegetate, and I therefore, sprinkled them plentifully with water in the three succeeding days; and at the end of ten days, I found them all diseased as in the preceding cases."

Somewhat similar experiments were carried out by SCHØLER, a Danish schoolmaster, in 1813-1817.

It was in Denmark also that the struggle between the practical farmers who wished to eradicate the barberry and those who defended the bush reached such heights as to amount almost to a bitter feud. The story of this struggle is told at some length by LIND (15).

Cereal Smuts

In his classic *Dissertation sur la Cause qui Corrompt et Noircit Les Grains de Bled dans les Épis; et sur les Moyens de Prevenir ces Accidens*, in which he reviews a number of the earlier opinions and theories as to the causes of various diseases of grains, MATHIEU TILLET (28) also gives the results of his own observations and experiments. TILLET in 1791 distinguished between la Carie, stinking smut, and le Charbon, loose smut. He even notes microscopic differences in what he calls the dust, that is, the spores, associated with the two diseases. To be sure TILLET did not at the time recognize smut as either a parasite or fungus but no one who reviews his text and notes the careful tabulations of his experiments in inoculating with bunt and in control by treating the seed with a solution of potassium nitrate and lime can fail to be impressed with the fact that it is not necessary to know the name of the fungus or even that one is dealing with a fungus to carry out thoroughly scientific inoculation and control experiments. TILLET clearly demonstrated the infective character of the smut "dust" by sowing clean and artificially infected seed in neighboring plats.

It remained, however, for BÉNÉDICT PRÉVOST (21) to demonstrate in 1807 by germination of the smut spores in water that they were actually fungi. Of perhaps even greater interest to pathologists is the fact that in this pioneer paper, PRÉVOST also carefully describes his methods of testing the efficiency of the fungicides by observing the inhibition that they cause in the germination of fungus spores in water solutions. He points out (page 60) that at low temperatures a solution of one part copper sulphate in four hundred thousand of water was sufficient to prevent the germination of smut spores, and one part in twelve hundred

thousand would retard germination. He also tested metallic copper and copper acetate in the same way, and gives descriptions for using both copper sulphate and copper acetate on a large scale.

Knowledge of smut as a disease and the use of means for its control are much older. During the early years of the century now under consideration, JETHRO TULL (29), the inventor of horse cultivation and drilling, remarked in his book *Horse-Hoeing Husbandry* that " Smuttiness is when the grains of wheat instead of flour, are full of a black, stinking powder." He also says that " Whatsoever the cause be, there are but two remedies proposed; and those are brining, and change of seed. " He gives the following account of the development of brining in England :

" Brining of wheat, to cure or prevent smuttiness (as I have been credibly informed), was accidentally discovered about seventy years ago, in the following manner : viz. a ship-load of wheat was sunk near Bristol in Autumn, and afterwards at Ebbs all taken up, after it had been soaked in Seawater ; but it being unfit for making of bread, a Farmer sowed some of it in a field ; and when it was found to grow very well, the whole cargo was bought at a low price by many Farmers, and all of it sown in different places. At the following harvest, all the wheat in England happened to be smutty, except the produce of this brined seed, and that was all clean from smuttiness. This accident has been sufficient to justify the practice of brining ever since in all the adjacent parts and in most places in England."

His directions for seed treatment are :

" The most expeditious way of brining wheat for the drill; is to make a very strong Brine ; and when the wheat is laid on an heap sprinkle or lave it therewith ; then turn it with a shovel, and lave on more brine ; turn it again with a shovel, until, by many repetitions of this, the wheat be all equally wet. Next, sift on quick-lime through a sieve ; turn the wheat with a shovel, and sift on more lime ; repeat this sifting and turning many times, which will make it dry enough to be drilled immediately ; and this have been found sufficient to preserve uninfected wheat from the smut in a bad year, the seed being changed."

TULL had no illusions as to brining being a cureall for he frankly

states that " Smutty seed-wheat, tho' brined will produce a smutty crop, unless the year prove very favourable." " But of the two remedies against smuttiness, a proper change of seed some think the most certain."

The possibility that the change of seed might have resulted in securing disease-free stock apparently did not occur to TULL, but his idea of the advantages of a change of land is indicated below :

" To know what changes are best to prevent smuttiness of wheat, we must consult the most experienced ; and they tell us, that the strong clay land is best to be sent to for seed-wheat, whatever sort of land it be to be sowed upon ; a whiteclay is a good change for a red-clay, and a red for a white. That from any strong land is better than from a light land ; and the old rhyme is that sand is a change for no land. But from whatever land the seed be taken, if it was not changed the preceding year, it may possibly be infected ; and then there may be danger, tho' we have it immediately from never so proper a soil."

Rotation of crops was recognized as a means of control of this disease for TULL plainly states :

" The strongest objection that has been yet made against constant annual crops of wheat, is, that those grains of the precedent crop which happen to head, and grow in the following crop, will be in danger of smuttiness, for want of change of those individual seeds."

He also notes " that a crop very early planted is not so apt to be smutty."

The usefulness of this practice did not go unchallenged, however, and in 1784, HENRY BRYANT (5) published a small volume chiefly for the purpose of showing that the seed treatment was not only ineffective but injurious to the seeds. It is very likely that at least some part of BRYANT's contentions were true especially when one realizes that there were used in seed treatment for smut not only sea-water but a mixture of wood ashes and urine, lime from the soap boiler, urine and cheese whey, arsenic and even sulphuric acid.

Answers to the objections raised by BRYANT are found in various papers including one by WILLIAM MARSHALL, who presented

results of field observations to prove that seed treatment is effective. The Reverend WILLIAM KIRBY gives the results of both his microscopical observations of the fungus which caused the disease and field observations on its control. He refers to the custom of seed treatment being nearly universal, at least in the eastern counties, and adds " But what kind of substance it is whether animal, vegetable, or merely a distemper incident to this grain agriculturists do not trouble themselves much to inquire : This indeed is properly the business of the naturalist and of these latter the opinions concerning it are various." It appears that the practical grower of this time had no need to concern himself greatly with the causes of this disease, since he had at hand what seemed to be a satisfactory control.

KIRBY (12) also quotes the experiments of a Mr. LATHBURY from which it appears that a simple washing of the seed with water if it be carefully wiped dry, answers the same end as steeping in a more costly preparation, although he admits that it could not be done with sufficient care and accuracy on a large scale.

ALEXANDER HUNTER (10) stated in 1803 that " The whole of this part of natural history is wrapt up in obscurity; but it is a singular happiness that the vegetative principle of smut may be destroyed by certain steeps well known to the farmer. These seem to operate in a twofold capacity, by destroying the germination of the smut, and by allowing the farmer to skim it off during the time of brining."

Pear Scab

Spraying (or as it was then called " syringing "), dusting, and fumigation were probably first developed as means of insect control, and it is of interest to note that all three had been suggested in print prior to the close of the eighteenth century (7). Both spraying and dusting had been employed against fungus diseases prior to 1830.

We find in the writings of the versatile THOMAS ANDREW KNIGHT (13), an account read before the British Horticultural Society in 1815 of the successful control of what was apparently pear scab by dusting the pear tree with flowers of sulphur which was found to injure the foliage less than the combination of lime

and sulphur. To be sure he referred to the fungus as *Lycoperdon cancellatum* but other investigators in other centuries have failed to satisfy the botanical public with the names used for the fungi they sought to control by spraying. KNIGHT's discussion of the disease clearly shows that he knew the fungus to be the cause of the disease and that it was " capable of being transferred from one plant to another in its vicinity by means of its seeds " or as we should say the " spores."

Control of Peach Leaf Curl with Lime and Sulphur

KNIGHT (14), in a paper, entitled, *Upon the Causes of the Diseases and Deformities of the Leaves of the Peach-Tree*, read July 15, 1834 distinguished between leaf curl and red spider injury and stated that his gardener applied " water, holding in solution or suspension a mixture of lime, and flowers of sulphur," when the blossom buds " had acquired about the size of hemp-seeds." This apparently was intended to prevent the ravages of red spider but was found to prevent the disease. KNIGHT avowed his inability to account for the latter fact or for the curl itself.

Control of Peach Mildew with Sulphur

In a communication to the Horticultural Society read November 20, 1821, on the *Mildew and some other Diseases incident to Fruit Trees*, Mr. JOHN ROBERTSON (24) describes the powdery mildew of peach, which he correctly attributes to the action of a parasitic fungus. He proves it is transmissible by inoculation and describes the successful control of the disease by timely applications of sulphur. This is prepared by mixing it with soap suds, and then applying by dashing it violently with a rose syringe against the tree and wall, in such a manner that no part or leaf is left unsprinkled.

VIRUS DISEASES

Peach Yellows

As Dr. ERWIN F. SMITH has shown in his careful review (27)

published in 1888, peach yellows first appeared in the country sometime prior to 1791. In 1796 the American Philosophical Society offered a premium for a method " verified by experiment " for the control of this disease. By 1806, Judge RICHARD PETERS had become convinced that the death of the trees was not due to worms but to what he referred to as " some morbid infection in the air." He further says :

" The disorder being generally prevalent would, among animals, have been called an epidemic. From perfect verdure the leaves turned yellow in a few [?] days, and the bodies blackened in spots. Those distant from the point of infection gradually caught the disease. I procured young trees from a distance in high health and planted them among the least diseased. In a few [?] weeks they became sickly, and never recovered. * * * * After my general defeat and most complete overthrow, in which the worm had no agency, I recruited my peaches from distant nurseries, not venturing to take any out of those in my vicinity. I have since experienced a few instances of this malady, and have promptly, on the first symptoms appearing, removed the subjects of it, deeming their cases desperate in themselves and tending to the otherwise inevitable destruction of others."

In 1828, Mr. WILLIAM PRINCE of Flushing, L.I., N.Y., advanced what he considered and what is still considered decisive proof of the infectious nature of this disease :

" A decisive proof of its being contagious is that a healthy tree inoculated from [i.e. grafted with] a branch of a diseased one, instead of restoring the graft to vigor and health, immediately becomes itself infected with the disease."

PRINCE also gave at that time the following practical directions for the control of this disease by eradication methods, which have, I believe, not been improved upon in 105 years :

" As soon as a tree is discovered to possess the characteristics of the disease, which is generally known by the leaves putting on a sickly, yellow appearance—but of which the premature ripening of the fruit is decisive proof—it should be marked, so as to be removed the ensuing autumn, which must be done without fail, for if left again to bloom, it would impart the disease to many others in its vicinity ; care is also

necessary in its removal to take out all the roots of the diseased tree, especially if another is to be planted in the same place, so that the roots of the tree planted may not come in contact with any of those of the one which was diseased.

"If your neighbor has trees infected with the yellows in a quarter contiguous to yours, it will be necessary to prevail on him to remove them, that yours may not be injured by them. By being thus particular in speedily removing such trees as may be infected, the disease is prevented from extending itself to the rest of the orchard and the residue will constantly be preserved in perfect health at the trifling loss of a few trees annually from a large orchard."

Virus Diseases of Potatoes

In a careful review of the early literature on the disease or group of diseases then known as "Curl," "Kräuselkrankheit," and "Frisolée," ATANASOFF (2) cites over 30 papers published prior to 1833 in German, French, or English. This literature clearly shows that there appeared in England and on the Continent of Europe somewhat before 1775 a peculiar disease on potato plants of which the most conspicuous symptoms were the stunting of the plants, curling and spotting of their leaves, and a marked and sudden decrease in yield. In some parts of Germany, for example, the yield in 1783 was only one-third to one-sixth of what had previously been considered normal.

During the few years following 1775 the disease appeared almost in an epidemic form so that it became the subject of general interest and concern in all the countries where it was found. A common explanation of the disease was that it was a degeneration of the potato plant from its continued asexual propagation. Another explanation was that it was a result of cross pollination of the ordinary potato variety used only as animal food. Other explanations were, the unfavorable soil and climate of Europe, and permitting the seed potatoes to become overripe.

These theories, however, were combatted with vigor and skill, and the following facts were known by students of the disease as early as 1788, perhaps indeeed as early as 1778 :

1. The disease was much less frequent in the Northern than in the Southern part of the British Isles.

2. The presence of the disease depended entirely on the nature of the seed, and was apparently perpetuated as long as the infected " breed " continued to reproduce.

3. That the disease could increase in a field planted with seed having a relatively light infection.

4. That in general potato plants affected with the curl seldom produced tubers comparable in size with those produced by healthy plants, with the result that in general large potatoes were more satisfactory for seed purposes.

5. That some degree of control could be secured by rogueing the potatoes as soon as they were above ground.

By 1802 it was also noted that the years in which the curl was most prevalent were those in which insects were very numerous.

Certainly by 1796 the annual purchase of seed-potatoes from Scotland as a control measure against curl was well established in England, as a result of an accidental discovery. About 1776, at a time when potatoes were high in England, a sloop load of potatoes from Scotland arrived in Liverpool late in the spring. These were used for planting and the resulting crops were so good and so free from curl, that such importation of seed potatoes soon became a regular practice, which still continues after a century and a half.

Nor was the information, above summarized, limited to only a few students, for in 1814 we find in the Memoirs of the Caledonian Society these two " simple rules " for combatting curl (8) :

1. " To procure a sound, healthy seed-stock, which can not be relied upon unless obtained from a part of the high country."

2. " To plant such potatoes as are intended to supply seed stock for the coming season, at least a fortnight later than those planted for crop."

This last precaution had the necessary result of separating the potatoes intended for seed from the rest of the crop. Ten years ago ATANASOFF suggested that this " curl " of 1775 to 1800 was streak or a combination of streak with some other virus diseases. It would be, indeed, a rash historian who in 1933 would venture to identify it with any one of the numerous virus diseases now recognized in the potato or even to insist that this was a virus disease. It is, however, evident that there was then recognized

a serious disease of potatoes, with symptoms strikingly like those now believed to be caused by certain virus diseases, and which was most effectively held in check by purchasing northern grown seed at least once in two years and growing the seed potatoes in a separate piece supplemented by rogueing when the plants are small. All of which coincides strikingly with the advice I heard given during the summer of 1932 to various groups of New York growers regarding the control of virus diseases of potato by one of the most experienced extension pathologists in the United States.

Mere coincidence of the ideas advanced 100 years ago with those of the present does not guarantee their accuracy but it certainly adds to their interest. The recent (April 1932) publication by the New Hampshire Agricultural Experiment Station of the observation that " Leafroll tends to increase with the maturity of the seed in southern New Hampshire," is closely paralleled by the statement made by JOHN YOUNG (31) in the third volume of the *Memoirs of the Caledonian Horticultural Society* in 1825 to the effect that " The best means of preventing the curl in potato is to take care that those intended for seed should not be too ripe."

THEORETICAL ASPECTS

Our predecessors of a century or more ago appear to the least advantage when they present their theories regarding the cause of disease. It would serve no useful purpose to detail or even to list the now discarded theories regarding plant disease prevalent during the century, such as that cereal smut was due to lack of fertilization arising from an incapacity of the male organs of generation, or that it is caused by insects; that the barberry was injurious to wheat because its pollen blew over on the wheat and poisoned it; that club root of cabbage was due to unfavorable weather or insects; or that peach yellows was due to worms. Too many of the theories advanced since 1900 are already overthrown for us to get up much enthusiasm for laughing at those of our predecessors. Perhaps, we might let him who is without sin in the form of a discarded theory cast the first stone of ridicule at these early theorists.

Finally, it may be said in defence of those earlier students that many of them were aware of their limitations. We find, for example, that the lack of close cooperation between the scientific investigator and practical man was recognized as a weakness. One author regrets that in dealing with rusts and smuts the investigations have been chiefly conducted by practical men not acquainted with science, or by scientific men ignorant of practical matters.

In the introduction to his Rural Economy of Norfolk, (17) in what he calls an " Address to the Reader " MARSHALL emphasizes the importance of more careful records in " rural economy " and the need of scientific knowledge as applied to agriculture. In a paper published five years after the close of the century, entitled, " On the Diseases Incident to the Most Usually Cultivated Plants," G. W. JOHNSON (11) warns against a too narrow outlook in the study of plant diseases and says that " Whatever may be the disease under which a plant is suffering, it is too usual for the cultivator to confine his attention to the part immediately affected," and with reference to frequent discussions regarding the cause of diseases he insists in words still pertinent that " Where there is much difference of opinion there is little real knowledge."

DISEASE CONTROL

By the close of the eighteenth and early years of the nineteenth century there were recognized as means of controlling plant diseases the following methods, all of which are still in good repute and regularly practiced :

1. The impregnation of timber with toxic salts to prevent decay.
2. Tree surgery, including the application of dressings.
3. Providing low temperatures with adequate humidity and ventilation for stored fruits and vegatables (the possibility of gas storage had been suggested but was not in general use, as indeed it is not in general use today).
4. Dusting and spraying with sulphur or lime and sulphur.
5. The eradication of the alternate host of a parasitic fungus.
6. Seed treatment with various salts (recognized as performing two functions, seed sterilization and the

mechanical removal of disease producing "germs").

7. Securing disease-free seed and propagating it separately.
8. Systematic rogueing.
9. The use of lime on the soil.
10. Fallow for the control of nematode diseases.
11. Regulating the proportions of various plant foods—notably nitrogenous.
12. Crop rotation.

Supplementing these there was of course the age long practice of noting the most susceptible varieties and propagating those which proved more resistant.

From this inadequate review it is evident that the plant pathology of the century which we celebrate developed on a foundation of accurate observation and sound practice established during the preceding 100 years. If in contrast to their observations and practices, many of the theoretical explanations of these early students seem fantastic, we may well reflect that inevitably some of the most emphatically enunciated theories of the present will appear fantastic long before the year 2033, and that their era like ours was a century of progress.

(Bureau of Plant Industry NEIL E. STEVENS.
Washington, D. C.)

LITERATURE CITED

The following is of course not a complete bibliography, it is not even a complete list of the literature mentioned in the preceding pages. The references given here are either to papers of rather general interest or such that are not readily identified by the descriptions contained in the text.

1. — ARTHUR, J. C. The plant rusts. 1929.
2. — ATANASOFF, D. A study into the literature on stipple-streak and related diseases of the potato. *Meded. Landbouwhoogeschoole Wageningen*, 26 (1): 1-52. 1922.
3. — BOULTON, HAROLD. A century of wood preserving. London. 1930.
4. — BROOKS, CHARLES et al. Effect of solid and gaseous carbon dioxide upon transit diseases of certain fruits and vegetables. *U. S. D. A. Tech. Bul.* 318. 1932.
5. — BRYANT, HENRY. A particular enquiry into the causes of that disease in wheat, commonly called brand. 1784.
6. — COXE, WILLIAM. A view of the cultivation of fruit trees. 1817.
7. — DEANE, SAMUEL. The New England farmer; or, Georgical dictionary. Second edition. 1797.

8. — Dickson, Thomas. Observations on the disease in the potato, generally called the curl. *Mem. Caledonian Hort. Soc.* 1 : 49-59. 1814.
9. — Forsyth, William. A treatise on the culture and management of fruit-trees. 1802.
10. — Hunter, Alexander. On the smut in wheat. In Hunter, A. Georgical Essays. York. 1803. vol. 6, p. 125-127.
11. — Johnson, G. W. On the diseases incident to the most usually cultivated plants. *Quart. Journ. Agr.* 8 (2) : 308-317. 1837.
12. — Kirby, William. Observations upon certain fungi, which are parasitic of the wheat. *Trans. Linn. Soc.* 5 : 112-125. 1809.
13. — Knight, T. A. On the mode of propagation of the Lycoperdon cancellatum, a species of fungus, which destroys the leaves and branches of the pear tree. *Trans. Hort. Soc. London.* I, 2 : 178-183. 1818. Paper read Dec. 5, 1815.
14. — Knight, T. A. Upon the causes of the diseases and deformities of the peach-tree. *Trans. Hort. Soc. London.* II, 2 : 27-29. 1842. Paper read July 15, 1834.
15. — Lind, J. Berberisbusken og berberisloven. *Tidsskr. Planteavl.* 22 : 729-780. 1915.
16. — Loudon, J. C. An encyclopedia of agriculture. 1825.
17. — Marshall, William. The rural economy of Norfolk. 1787.
18. — Marshall, William. The rural economy of the midland counties. Ed. 2. 1796.
19. — Needham, J. T. Worms discovered in smutty corn. *Phil. Trans. Roy. Soc.* London 42 : 634-641. 1744.
20. — Plenck, J. J. Physiologia et pathologia plantarum. Vienna. 1794.
21. — Prévost, Bénédict. Mémoire sur la cause immédiate de la carie ou charbon des blés et de plusieurs autres maladies des plantes. 1807.
22. — Ramsbottom, John. Some notes on the history of the classification of the Uredinales. *Trans. Brit. Myc. Soc.* 4 : 77-105. 1913.
23. — Ricker, P. L. A valuable unpublished work on pomology. *Science* N. S. 44 : 62-64. 1916.
24. — Robertson, John. On the mildew and some other diseases incident to fruit trees. *Trans. Hort. Soc. London.* I, 5 : 175-185. 1824. Read Nov. 20, 1821.
25. — Roffredi, D. M. Mémoire sur l'origine des petits vers anguilles du blé rachitique. *Observ. Mém. Phys. Hist. Nat.* 5 : 1-19. 1775.
26. — Schwerz, J. N. Anleitung zum practischen Ackerbau. Stuttgart. Edition 1. 1825.
27. — Smith, E. F. Peach yellows. *U. S. D. A. Botanical Division Bull.* 9. 1888.
28. — Tillet, Mathieu. Dissertation sur la cause qui corrompt et noircit les grains de bled dans les épis, et sur les moyens de prévenir ces accidens. Bordeaux. 1755.
29. — Tull, Jethro. Horse-Hoeing husbandry. Second Edition. 1733.
30. — Whetzel, H. H. An outline of the history of phytopathology. Philadelphia. 1918.
31. — Young, John. On the cause of the curl in the potato, and the means of prevention. *Mem. Caledonian Hort. Soc.* 3 : 278-286. 1825.
32. — Zallinger, J. B. De morbis plantarum cognoscendis et curandis dissertatio ex phaenomenis deducta. 1773.

A Missing Chapter in the History of Organic Chemistry: The Link between Elementary Analysis by Dry-distillation and Combustion

Prior to the introduction of elementary analysis by combustion (1), organic matter was analysed over a period of nearly two hundred years by dry-distillation (2). The results obtained by this method were recorded, where possible, in weighed fractions referred to either as the gaseous part, the phlegma, the oil and the carbon residue, or later on as the carbonic oxide and carbonic acid fractions, the watery fraction, the emphyrematic oil, the acidic fraction, the carburated hydrogen fraction and the charcoal residue. Reference was also made to the volatile alkalies or later on to nitrogen and ammonia.

With the introduction of elementary analysis the results came to be expressed either in percentages or in ratios of carbon, hydrogen, oxygen and nitrogen, from which the empirical formulae

---

(1) For a good historical survey of elementary analysis by combustion see DENNSTEDT, " Die Entwickelung der organischen Elementaranalyse," AHREN's *Sammlung chem. u. chem.-techn. Vorträge*, 4, pp. 1-114 (1899), where, however, no reference is made to the historically important preliminary communication by GAY-LUSSAC, " Über die Analyse vegetabilischer und thierischer Substanzen...," GEHLEN's *Journal für die Chemie...* 9, 763 (1810), in which GAY-LUSSAC's method is described for the first time.

(2) The germ of elementary analysis by dry-distillation is already to be found in BEGUIN, *Les Élémens de Chymie*, pp. 271-279, Paris (1615), and so far as it has been possible to ascertain the last investigator to use this method was FOURCROY, *Ann. de Chimie*, 3, 114 (1794). Detailed summaries of the results obtained during this period (1615-1794) of nearly two hundred years are given in GMELIN, *Geschichte der Chemie...* 3 vols., Göttingen (1797-1799). The publication by MARGGRAF, " Chymische Untersuchung des Ceder-Holzes...", *Chymische Schriften*, vol. i, pp. 233-240, Berlin (1768), is a remarkably exact investigation by this disused method and deserves our admiration.

were evolved in due course. Elementary analysis by combustion entirely replaced analysis by dry-distillation and practically no attempt seems to have been made (3) to co-ordinate the results obtained by these two methods. This is remarkable considering the vast amount of work which had been done using dry-distillation as a method of analysis. Still more remarkable is the fact that this has not been commented on by writers on the history of chemistry in general (4) and of organic chemistry (5) in particular. It seems as if history accepted the sudden disappearance of analysis by dry-distillation in favour of elementary analysis by combustion without any stand on behalf of the former, an event so foreign to the usual progress of scientific thought.

Casual references by RUNGE (6) to what he terms " Phyto-Stöchiologie," and the examination of the works on plant chemistry published from 1800 to 1825 listed by EBLE (7), show however that long after the introduction of elementary analysis by combustion the results obtained by dry-distillation were still considered seriously in some quarters, especially by those RUNGE terms the " Phytochemiker," who thus provide the to-be-expected link between the two analytical methods for the analysis of organic matter.

In spite of this interest only one work on these lines seems

(3) The paper by FOURCROY already mentioned contains a very half-hearted attempt to co-ordinate the results obtained by him by the two methods. This seems to be the only case on record.

(4) KOPP, *Geschichte der Chemie*, vol. iv, pp. 248-263, Braunschweig (1847); HOEFER, *Histoire de la Chimie*, deuxième édition, vol. ii, pp. 524-535, Paris (1869); JAGNAUX, *Histoire de la Chimie*, vol. ii, pp. 451-469, Paris (1891); HIORTDAHL, *Fremstilling af Kemiets Historie*, vol. iii, pp. 11-28, Christiana (1907).

(5) HJELT, *Geschichte der organischen Chemie...* pp. 44-61, Braunschweig (1916); GRAEBE, *Geschichte der organischen Chemie*, pp. 11-24, Berlin (1924).

(6) RUNGE, *Materialien zur Phytologie*, 2 vols., Berlin (1820, 1821). Next to WAHLENBERG's *De Sedibus materiarum immediatarum in plantis tractatio...* Upsaliae (1806, 1807), translated into German by HILDEBRANDT in GEHLEN's *Journal für die Chemie...* 8, pp. 92-151 (1809), RUNGE's work is one of the earliest organised attempts at the study of plant chemistry, for which he advocates in the preface to vol. i the formation of a " phytochemische Gesellschaft " and stipulates " dass keiner Mitglied werden kann, der sich nicht wenigstens durch einige phytochemische Entdeckungen dieser Auszeichnung würdig gemacht hat." RUNGE adds with reference to the suggested Society : " sie wird so immer klein, aber umso kräftiger seyn."

(7) EBLE, *Curt Sprengel's Versuch einer pragmatischen Geschichte der Arzneikunde fortgesetzt...* vol. i, p. 163, Wien (1837).

to have been published, namely, *Die Entwickelung der Pflanzensubstanz...* by NEES VON ESENBECK, BISCHOF and ROTHE, the full title of which is reproduced in this article (p. 129). This work seems to have had no influence whatever on the progress of organic chemistry (8) and it is hard to realise how organic chemistry would have developed had not this work been stillborn (9).

Die Entwickelung der Pflanzenstoffe... consists of the correspondence of NEES VON ESENBECK (1776-1858), C. G. BISCHOF (1792-1870) and H. A. ROTHE (1773-1842). All three held prominent positions in their particular branch of science. NEES VON ESENBECK was known for his *System der Pilze und Schwämme* (1816), BISCHOF, for his *Lehrbuch der Stöchiometrie* (1819) and ROTHE for his two works *Handbuch der reinen Mathematik*, 2 vols. (1804, 1811), and *Theorie der combinatorischen Integrale* (1819). *Die Entwickelung der Pflanzenstoffe...* opens with a letter (pp. 1-21) by NEES VON ESENBECK addressed to BISCHOF inviting him to work out a stecheochemistry of the plant constituents. The letter is headed " Speculation " and has the following quotation from Sir FRANCIS BACON : " Magnalia naturae fere extra vias tritas et orbitas notas jacent." It is an interesting philosophical essay on plant physiology and a good companion to NEES VON ESENBECK's *Naturphilosophie* (10). This is followed by a letter (pp. 23-60) from BISCHOF to NEES VON ESENBECK, headed " Empirie," with a quotation from PLINY : " Res ardua, — novis auctoritatem dare, — dubiis fidem." In this letter BISCHOF suggests the application of stecheochemical principles to the analytical data obtained by combustion as based on the data obtained by dry-distillation, each fraction forming a separate unit. Historically BISCHOF's letter is of particular interest since it gives a concise résumé of the knowledge of plant chemistry of the

(8) There are only a few references to be found to this book : RUNGE, *loc. cit.*, vol. i, p. 3, where it is described as " ein für die Pflanzenchemie wichtiges Ereignis "; EBLE, *loc. cit.*; and POGGENDORFF, *Biographisch-Literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften...* vol. i, col. 201 (article on BISCHOF) and vol. ii, col. 702 (article on ROTHE), Leipzig (1863).

(9) The writer experienced some difficulties in finding a copy and he wishes to thank Mr. H. ZEITLINGER of Messrs. HENRY SOTHERAN, Ltd., London, for the trouble he has taken in the matter. It is interesting to note that the copy he obtained arrived in its original unfolded sheets.

(10) NEES VON ESENBECK, *Das System der speculativen Philosophie*, Erster Band : *Naturphilosophie*, Glogau (1841).

time, summarising amongst other works the results obtained by the lesser known Dutch investigators such as DEIMANN, VAN TROSTWYK and LAUWRENBURGH.

The third section, which comprises the main part of the work, consists chiefly of the correspondence of BISCHOF and ROTHE, finishing up with a letter from BISCHOF to NEES VON ESENBECK explaining the mathematical data obtained by ROTHE, and applying them to a number of cases of published results by BERZELIUS, GAY-LUSSAC, SAUSSURE and PROUST. This section (pp. 61-232) is headed " Mathematik " and has the following quotation from JUVENAL : " Nunquam aliud Natura, aliud Sapientia dixit."

It will be realised that the most important share of this joint work belongs to ROTHE, who finally elaborated a series of 31 tables, one of which is reproduced on p. 130, in which the results obtained by the combustion method are expressed in " complexes " of carbon oxide, carbon dioxide, water, the oilproducing fraction (emphyrematic oil) and the carburated hydrogen fraction. ROTHE's " Complex Tabellen " correspond to our modern RICHTER's tables (11), so well known to the present generation of organic chemists. The principle underlying them is best explained by the following quotation (p. 132) from a letter written by ROTHE to BISCHOF :

" Wir lassen nun die mit vieler Mühe berechneten Tafeln folgen, welche alle oben (S. 69-75) angegebenen 961 Complexionen der fünf binären Verbindungen des Sauerstoffs, Wasserstoffs und Kohlenstoffs enthalten. Es bezeichnet :

a einen Verhältnisstheil (Massentheil, Atom) Wasser;
b einen Verhältnisstheil (Massentheil, Atom) Kohlenoxydgas;
c einen Verhältnisstheil (Massentheil, Atom) Kohlensäure;
d einen Verhältnisstheil (Massentheil, Atom) Oelerzeugendes Gas;
e einen Verhältnisstheil (Massentheil, Atom) Kohlenwasserstoffgas.

" Um die Tafeln nicht zu weitläufig zu machen, gingen wir bloss bis auf drei Verhältnisstheile.

" Zur Erklärung dieser Tafeln fügen wir nur noch folgendes bei. Die erste Spalte enthält die fortlaufenden Nummern der Complexionen. Die zweite die Nummern der mit jenen gleich-

(11) RICHTER, *Prozent-Tabellen der Kohlenstoff-Verbindungen...* Leipzig and Hamburg (1912).

geltenden Complexionen, d.h. derjenigen, welche zwar an sich verschieden sind, aber doch auf einerlei Verhältniss der Grundstoffe führen. Da nur ein einziges Mal der Fall eintrat, wo 21 solche gleichgeltende Complexionen gefunden wurden, und die Nummern dieser 21 Complexionen wegen Mangel an Raum nicht füglich in der zweiten Spalte aufgeführt werden konnten: so haben wir bei diesen Complexionen, welche die mit den Nummern 55, 127, 188, 235, 242, 304, 311, 380, 387, 449, 456, 503, 568, 575, 643, 705, 712, 759, 824, 831, 900 bezeichneten sind, statt derselben den Buchstaben S gesetzt. Die dritte Spalte enthält die Zusammensetzungen der binären Verbindungen selbst, in der Ordnung, wie die Complexionen oben (S. 69-75) auf einander folgen, und zwar so, dass das letzte Element am weitesten zur Rechten mit *a*, das vorletzte zunächst zur Linken mit *b* u. s. w. als Coëfficient verbunden ist. Die vierte Spalte enthält die Summe der in der fünften, sechsten und siebenten angegebenen Verhältnisszahlen des Sauer- Wasser- und Kohlenstoffs; es sind daher diese Zahlen in der vierten Spalte zugleich die stöchiometrischen Werthe von den in der dritten befindlichen Zusammensetzungen der binären Verbindungen. Endlich enthalten die drei letzten Spalten die Reduction der Verhältnisszahlen der drei vorhergehenden Spalten auf die Summe 100."

One example, from the covering letter to ROTHE's tables written by BISCHOF to NEES VON ESENBECK, may serve as an illustration as to the use of these tables. The example in question refers to the analysis of "Milchzucker" by BERZELIUS (p. 173), who found C = 45.15; H = 6.17; O = 48.68 per cent. These values correspond to complexes 187, 303, 386, 502, 703 and 898 in ROTHE's tables. The formulae arrived at are therefore $C_{30}H_4O_{32}$ (complexes 187, 303, 386 and 502), and $C_{60}H_8O_{64}$ (complexes 703 and 898), made up as follows:

No. 187. $3CH + CO_2 + CO + OH$.
No. 303. $CH_2 + CH + 3CO + OH$.
No. 386. $CH_2 + 2CH + 2CO_2$.
No. 502. $2CH_2 + CO_2 + 2CO$.
No. 703. $2CH_2 + 3CH + 2CO_2 + 3CO + OH$.
No. 898. $3CH_2 + 2CH + 3CO_2 + 2CO$.

In ROTHE's tables $a = OH$ (water); $b = CO$; $c = CO_2$; $d = CH$ and $e = CH_2$, where C = 6; H = 1; O = 8 (p. 63).

In addition to the tables ROTHE also describes a series of formulae by which the complexes given in the tables can be extended. For BERZELIUS's "Milchzucker" 34 more complexes can thus be calculated, in addition to the 6 complexes shown in the tables.

The University, Bristol. M. NIERENSTEIN.

Die

ENTWICKELUNG

der

PFLANZENSUBSTANZ

physiologisch, chemisch und mathematisch dargestellt

mit

combinatorischen Tafeln der möglichen Pflanzenstoffe und den Gesetzen ihrer stöchiometrischen Zusammensetzung.

Herausgegeben

von

Dr. Christian Gottfried Nees von Esenbeck,

ordentlichem Professor der Botanik auf der Königlich Preufsischen Rheinuniversität zu Bonn, Präsidenten der Kaiserlich Leopoldinisch-Carolinischen Akademie der Naturforscher u. s. w.

Dr. Carl Gustav Bischof,

Professor auf der Königlich Preufsischen Rheinuniversität zu Bonn, Adjuncten des Präsidii der Kaiserlich Leopoldinisch-Carolinischen Akademie der Naturforscher u. s. w.

Dr. Heinrich August Rothe,

ordentlichem Professor der Mathematik auf der Königlich Baierischen Universität zu Erlangen, Mitgliede der Kaiserlich Leopoldinisch-Carolinischen Akademie der Naturforscher u. s. w.

Erlangen

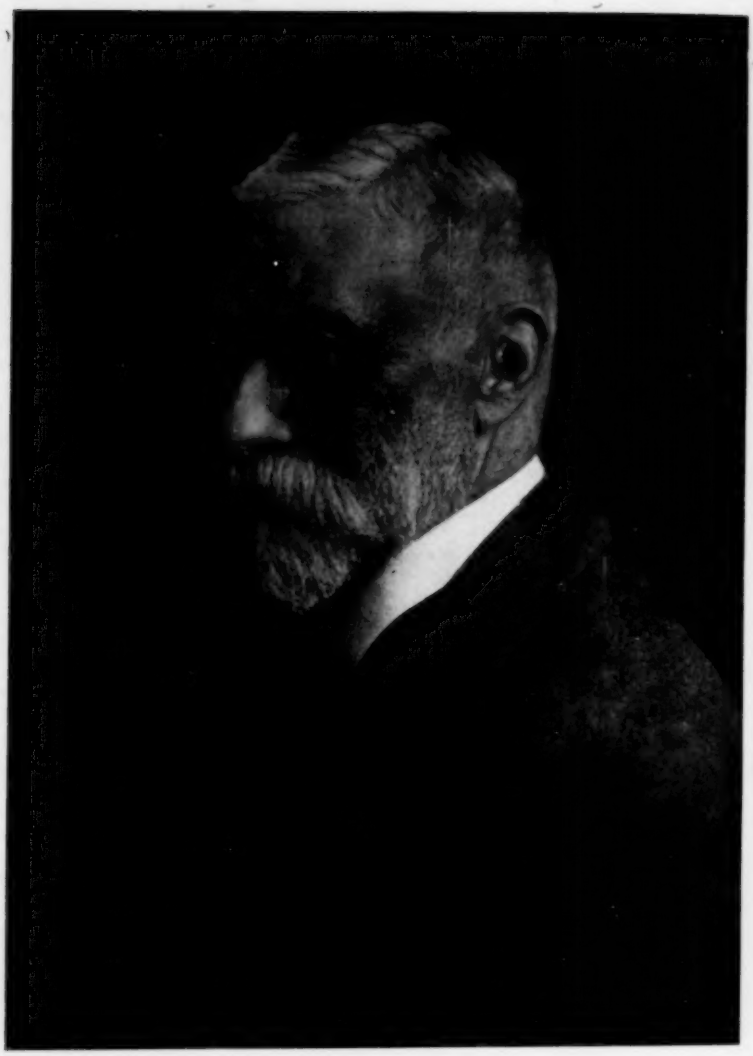
bei J. J. Palm und Ernst Enke.

1819.

Title-page of *Die Entwickelung der Pflanzensubstanz*.

Nummer	Nummer der gleichgeltenden Complexionen	Fortlaufende Complexionen	Stöchiometrische Zahlen				Nach 100 Theilen berechnet		
			Summe	Sauerst.	Wasserst.	Kohlenst.	Sauerst.	Wasserst.	Kohlenst.
187	303,386,502,703,898	3d+c+b+a	60	32	4	30	48,49	6,06	45,45
188	S	3d+c+b+2a	75	40	5	30	53,33	6,67	40,00
189	305,388,504	3d+c+b+3a	84	48	6	30	57,14	7,14	35,72
190		3d+c+2b	71	32	3	36	45,07	4,23	50,70
191	390,506	3d+c+2b+a	80	40	4	36	50,00	5,00	45,00
192	391,507,586	3d+c+2b+2a	89	48	5	36	53,93	5,62	40,45
193	392,508,587	3d+c+2b+3a	98	56	6	36	57,14	6,12	36,74
194		3d+c+3b	85	40	3	42	47,06	3,53	49,41
195	394	3d+c+3b+a	94	48	4	42	51,06	4,26	44,68
196	395,590	3d+c+3b+2a	103	56	5	42	54,37	4,85	40,78
197	396,591	3d+c+3b+3a	112	64	6	42	57,14	5,36	37,50
198	123,314	3d+2c	65	32	3	30	49,23	4,62	46,15
199	124,315,512,839	3d+2c+a	74	40	4	30	54,05	5,41	40,54
200	125,316,513,841	3d+2c+2a	83	48	5	30	57,83	6,02	36,15
201	317,514	3d+2c+3a	92	56	6	30	60,87	6,52	32,61
202	318	3d+2c+b	79	40	3	36	50,63	3,80	45,57
203	59,246,319,402,475	3d+2c+b+a	88	48	4	36	54,55	4,54	40,91
204	320,403,516	3d+2c+b+2a	97	56	5	36	57,73	5,16	37,11
205	60,247,321,404	3d+2c+b+3a	106	64	6	36	60,38	5,66	33,96
206		3d+2c+2b	93	48	3	42	51,61	3,23	45,16
207	406,518	3d+2c+2b+a	102	56	4	42	54,90	3,92	41,18
208	407,519	3d+2c+2b+2a	111	64	5	42	57,66	4,50	37,84
209	408,520	3d+2c+2b+3a	120	72	6	42	60,00	5,00	35,00
210		3d+2c+3b	107	56	3	48	52,34	2,80	44,86
211	63,66,139,250,330,410	3d+2c+3b+a	116	64	4	48	55,17	3,45	41,38
212	411	3d+2c+3b+2a	125	72	5	48	57,60	4,00	38,40
213	64,142,251,412	3d+2c+3b+3a	134	80	6	48	59,70	4,48	35,82
214	140,331,526	3d+3c+a	96	56	4	36	58,33	4,17	37,50
215	141,332,527	3d+3c+2a	105	64	5	36	60,95	4,76	34,29
216	334	3d+3c+b	101	56	3	42	55,45	2,97	41,58
217	335,530	3d+3c+b+a	110	64	4	42	58,18	3,64	38,18

Die Entwickelung der Pflanzensubstanz (1819)
One of the 31 tables of ROTHE.



J. L. E. DREYER
(1852-1926)

R. A. SAMPSON
& F. E. BRASCH

John Louis Emil Dreyer
(1852-1926)

Astronomy is sometimes called a microcosm of the sciences. It is indeed a mirror of history, for it is connected with many other lines of advance,—not least with the history of the development of ideas. But because it is merely one among many of man's activities, there are never more than a few historians that work at it, and sometimes there are none at all, so that it is right to mark one when he is found.

DREYER, as one remembers him in later life, was exactly shaped for the part. With him it was the effect of a life-long love of truth with a level and unprejudiced nature, coupled with the gradually accumulated stores of learning and experience.

The traditions of his family, which his sons have repeated, were military. His father was a lieutenant-general in the Danish army, and several of his ancestors were in the fighting services or public services of Denmark. But he, even at an early age, at the University of Copenhagen, showed a bent for learning. His first pamphlet was published when he was no more than 19; it was on the subject of his great compatriot TYCHO BRAHE,—a subject to which he returned twice afterwards.

The astronomical posts which DREYER held were not intrinsically important. He became astronomer to the Earl of Rosse in 1874. The earl who built the great reflector at Parsonstown, had died in 1867, and the telescope itself dates from 1845. It must have been of good quality in its day, but speculum metal, of which the mirror was made, though very permanent, deteriorates ultimately. Thus it hardly gave an opportunity to DREYER to become an observational astronomer of the first rank; nor in his subsequent posts had he ever control of the latest and best instruments. Like KAPTEYN, he turned this defect into an advantage, at least an advantage to us. It would be a mistake to imagine him as a

student only; but his most valuable work was done in the study. His contributions to general astronomy were two, very different in type. The first was a critical determination of the constant of precession, from the observed motions of the fainter stars, a work which required labour and judgment to execute, and which, though from its nature it is not widely known, received the high certificate of NEWCOMB's approval. The second is the indispensable and universally employed *New General Catalogue of Clusters and Nebulae*, which was first published in 1888 and contains 7840 entries to be followed by two supplementary catalogues, the former containing 5386 additions to the known lists up to 1907, and the second, a revision of Sir WILLIAM HERSCHEL's three catalogues. To execute these in a reliable way, very much more was required than making a list; the same object is variously described by different authors; the recorded places must all be brought up to one epoch; and so forth. It was a heavy piece of work, and as he did it, it has proved a very useful one.

But the best known of DREYER's works were his contributions to astronomical history. In 1878 he left Parsonstown, and became assistant at Dunsink. Here for a short period he was a colleague of COPELAND, whom he retained as a friend during their joint lives. They started together an astronomical journal,—*Urania* was its original name, but this was soon afterwards changed to *Copernicus*. This journal did not survive very long, but it contains several articles of high quality, many of them from DREYER's pen. Among them is the determination of the constant of precession, referred to above. There is also a correct description of the Chinese instruments, preserved at Pekin, both those of the 17th century, and the less known earlier ones of the 13th century. From Dunsink, he removed in 1882 to take charge of the observatory at Armagh, where he remained for the greater part of his life; in 1916 he was relieved of these duties in order that he might devote himself to the national republication of the works of TYCHO BRAHE; he thereupon removed to Oxford where he remained until his death.

In 1890 he produced his volume *Tycho Brahe : a Picture of Scientific Life in the Sixteenth Century*. We regard TYCHO's period rather as the end of an epoch than as a beginning; but

in reality, he was a link, uniting the modern to the mediaeval. Certainly it would not be possible to tell the story of any more central figure in science, for he was very widely known, and he lived in stirring times, and there is much to be ascertained about him and his friends, because at that epoch it was the custom of people to write long letters to one another. This book is a difficult and extensive piece of coordination, and one finds one may rely implicitly upon any statement that DREYER makes.

In the Armagh period, in 1906, came the work *History of Planetary Theories from Thales to Kepler*. This is a learned and useful compendium, derived from the original documents, with a critical facing of all the obscure points they involve. Others have done the same thing in parts, and also in parts at greater length, but it is doubtful if any more useful discussion exists.

DREYER took up again TYCHO's works. The editions of his writings are incomplete and very inaccurate. DREYER had long projected replacing them, and had obtained a grant from the Carlsberg Institute for the purpose. The scope was greatly increased, amounting to a transformation of it, by the private munificence of two men. The result is contained in the great national edition of TYCHO's works, of which 12 quarto volumes were published during DREYER's life. Fortunately he had completed the manuscript of the remainder, and these, together with an index volume, have since been issued. They contain very numerous notes and solutions of obscurities by DREYER, made with his invariable accuracy and love of truth, as well as an extensive prolegomenon on TYCHO's life. The whole has been turned uniformly into Latin.

DREYER was a man who got through a great deal of work. Originally a mere member of a committee of editors, he became ultimately the chief editor of the republication of Sir WILLIAM HERSCHEL's *Papers*, contributing to them a valuable introduction upon HERSCHEL's life, in which, as usual, he traced down and corrected numerous fictions that are current. In the same way he became also chief contributor and chief editor of the historical volume with which the Royal Astronomical Society marked its centenary,—not because he ever thrust himself forward, but because others found that he could, and was willing, to do the work.

DREYER received the Gold Medal of the Royal Astronomical

Society in 1916, and was President of the same Society in 1923, 1924. He held the Honorary Degree of D. Sc. from Belfast and that of M. A. from Oxford.

Edinburgh. R. A. SAMPSON.

Editorial footnote. The portrait attached to this obituary is due to Mr. BRASCH's courtesy. In a letter addressed to Mr. BRASCH, from H. M. S. Hood, Portsmouth, August 1927, DREYER's son, Vice-Admiral Sir FREDERIC CHARLES DREYER, stated "The photograph is an excellent one."

John L. E. Dreyer 1852-1926
Bibliography of his writings

TYGE BRAHE, en historisk fremstilling efter trykte og utrykte kilder af F. R. FRISS. kritisk betragtet af J. DREYER. Kjobenhavn, 1871. (19 p.)

Sur l'orbite de la première comète de l'an 1870. *Astr. Nachr.* 80, p. 219-222, 1872.

Beobachtung der Sonnenfinsterniss in Kopenhagen. *Astr. Nachr.* 81, p. 367, 1873.

Ueber eine bemerkenswerthe Deutung der Nebenradianten des Perseiden-Systems. *Astr. Nachr.* 82, p. 289-292, 1873.

Beobachtungen des Cometen c 1874 (Coggia). *Astr. Nachr.* 85, p. 71, 1875.

Note on Coggia's comet. (III. 1874) *Roy. Astr. Soc., Month. Not.*, 36, p. 339-340, 1876.

Request to astronomers. (Concerning observations for Sir W. HERSCHEL's cat.) *Astr. Nachr.* 88, p. 359, 1876.

Distribution of the fixed stars. *Observatory*, 1, p. 216, 1877.

On personal errors in astronomical transit observations. *Roy. Irish Acad., Proc.*, 2, p. 484-528, 1877.

Note on some of M. STEPHAN's new nebulae. *Roy. Astr. Soc., Month. Not.*, 37, p. 427-428, 1877.

Spiral form of nebulae. *Observatory*, 1, p. 370-371, 1878 : 2, p. 22-23, 1879.

Supplement to Sir JOHN HERSCHEL's "General catalogue of nebulae and clusters of stars." *Roy. Irish Acad., Trans.*, 26, p. 381-426, 1878.

Aspect of Mars in 1877. *Astr. Nachr.*, 94, p. 31-32, 1879.

An apparently new variable star. *Roy. Astr. Soc., Month. Not.*, 39, p. 374, 1879.

The radiant-point of Comet I., 1870. *Observatory*, 3, p. 120, 1879.

A record of the progress of astronomy during the year 1879. *Roy. Dublin Soc., Sci. Proc.*, 2, p. 437-483, 1880.

Copernicus, an international journal of astronomy. Edited by RALPH COPLAND and J. L. E. DREYER. 3 volumes. Dublin, 1881-1884.

Comparison stars, a suggestion to astronomers. *Copernicus*, 1. p. 49, 1861.

Meridian observations of Comet b, 1881. *Copernicus*, 1, p. 163, 1881.

Meridian observations of Comet c, 1881. *Copernicus*, 1, p. 181, 1881.

Note on the nebula near Merope. *Copernicus*, 1, p. 156-157, 1881.

Historical note concerning the discovery of Neptune. *Copernicus*, 2, p. 62-64, 1882.

Mean places of 321 red stars : deduced from observations made with the meridian circle at Dunsink. (85 p.) Dublin, HODGES, FIGGIS and co., 1882.

A new determination of the constant of precession. *Copernicus*, 2, p. 135-155, 1882. *Roy. Irish Acad., Proc.*, 3, p. 617-623, 1883.

Notes on the physical appearance of the planet Mars, as seen with the three-foot reflector, at Parsonstown, during the opposition of 1877. *Roy. Dublin Soc., Sci. Trans.*, 1, p. 69-70, 1883.

The Armagh Observatory. *Observatory*, 6, p. 239-243, 1883.

Note on the meteors connected with the first comet of 1870. *Roy. Irish Acad.*, Proc., 3, p. 255-256, 1883.

The instruments in the old observatory at Peking. *Roy. Irish Acad., Proc.*, 3, p. 468-472, 1883. *Copernicus*, 1, p, 134-138, 1881.

Historical account of the Armagh Observatory. Printed for private circulation, Liverpool, (20 p.) 1883.

Note on the accuracy of LALANDE's right ascensions. *Copernicus*, 3, p. 103, 1884.

On the multiple tail of the great Comet of 1744. *Copernicus*, 3, p. 104-111, 1884.

Further note on the Comet of 1744. *Copernicus*, 3, p. 184-185, 1884.

Observation of the transit of Venus, December 6, 1882 at Armagh Observatory. *Copernicus*, 3, p. 18-19, 1884. *Roy. Dublin Soc., Sci. Proc.*, 4, p. 49-50, 1885.

Occultations of stars and planets by the moon, observed at the Armagh Observatory from 1833 to 1844. *Astr. Nachr.* 110, p. 5-8, 1885.

On the supposed proper motion of the nebula h 705. *Observatory*, 8, p. 175-176, 1885.

Notice of a new catalogue of nebulae. *Astr. Nachr.*, 112, p. 42, 1885.

Comments on recent articles by Dr. HARTWIG in *Astr. Nachr.* concerning the appearance of Great Nebula in Andromeda. *Astr. Nachr.* 113, p. 270, 1886.

Second Armagh catalogue of 3300 stars for the epoch 1875, deduced from observations made at the Armagh observatory during the years 1859 to 1883, under the direction of T. R. ROBINSON, astronomer of the observatory. Dublin, THOM & co., (limited edition), 159 p., 1886.

Proposed change in the astronomical day. *Observatory*, 9, p. 130-131, 1886.

On the invention of the sextant. *Astr. Nachr.* 115, p. 33-36, 1886.

On the proper motion of twenty-nine telescopic stars. *Roy. Astr. Soc., Month. Not.*, 46, p. 379-391, 1886.

Note on the electric illumination of the Armagh refractor. *Roy. Astr. Soc., Month. Not.*, 47, p. 117-18, 1887.

On some nebulae hitherto suspected of variability or proper motion. *Roy. Astr. Soc., Month. Not.*, 47, p. 412-421, 1887.

Note on the effect of refraction in stellar photography. *Roy. Astr. Soc., Month. Not.*, 47, p. 421-422, 1887.

Occultations of stars during the total eclipse of the moon, January 28, 1888, observed at the Armagh Observatory. *Roy. Astr. Soc., Month. Not.*, 48, p. 268, 1888.

Note on the nebula h 347. *Astr. Nachr.* 120, p. 111, 1889.

The Astronomical Observatory of Pekin. *Nature*, 39, p. 55, 1889.

Schreiben betr. den weissen Fleck auf dem Ring des Saturn. *Astr. Nachr.*, 121, p. 215-216, 1889.

TYCHO BRAHE, a picture of scientific life and work in the sixteenth century. Edinburgh, A. & C. BLACK, (405 p.), 1890.

A new general catalogue of nebulae and clusters of stars, being the catalogue of the late Sir JOHN F. W. HERSCHEL, Bart., revised, corrected and enlarged. *Roy. Astr. Soc., Mem.*, 49, p. 1-237, 1890.

Uncataloged comets. *Observatory*, 14, p. 130, 1891.

Astronomical Bibliography. *Observatory*, 14, p. 322-325, 1891.

Proper motions of twenty southern stars. *Roy. Astr. Soc., Month. Not.*, 51, p. 466-474, 1891.

Who discovered the optical property of lenses? *Astr. Soc. Pacific, Publ.*, 3, p. 240, 1891.

A rare phenomenon. *Nature*, 44, p. 541, 1891.

Note on some apparently variable nebulae. *Roy. Astr. Soc., Month. Not.*, 52, p. 100-103, 1891.

Micrometric observations of nebulae made at the Armagh Observatory. *Roy. Irish Acad., Trans.*, 30, p. 513-558, 1894.

Note on the group of nebulae NGC. 3743-58. *Astr. Nachr.*, 136, p. 93-94, 1894.

Note on the solar eclipse of 1598. *Roy. Astr. Soc., Month. Not.*, 54, p. 439-441, 1894.

TYCHO BRAHE. Ein Bild wissenschaftlichen Lebens und Arbeitens im 16. Jahrhundert. Autoris. deutsche Uebersetzg. v. M. BRUHNS. 434 p. m. Bildniss. Karlsruhe, G. BRAUN, 1894.

The alleged comet of 1525. *Observatory*, 18, p. 164, 1895.

The eclipse of 1030 A.D. *Observatory*, 18, p. 363-364, 1895.

Index catalogue of nebulae found in the years 1888 to 1894, with notes and corrections to the New General Catalogue. *Roy. Astr. Soc., Mem.*, 51, p. 185-228, 1895.

Observations of the partial solar eclipse of 1895, March 25, at the Armagh Obs. *Roy. Astr. Soc., Month. Not.*, 55, p. 338, 1895.

On systematic errors in observing right ascensions of nebulae. *Roy. Astr. Soc., Month. Not.*, 57, p. 44-50, 1897.

Obituary notice of ALBERT MARTH. *Astr. Nachr.* 144, p. 223-224, 1897.

Recent publications on Mars. *Observatory*, 21, p. 85-88, 1898.

Observations of Mars in 1896-97. *Observatory*, 21, p. 346-347, 1898.

The partial eclipse of the Sun, May 28, 1900, observed at Armagh. *Roy. Astr. Soc., Month. Not.*, 60, p. 594, 1900.

Note on Colonel TUPMAN's paper on TYCHO BRAHE's meridian observations of the Sun. *Observatory*, 23, p. 211-212, 1900.

Corrections to the Armagh Catalogue for 1840. *Roy. Astr. Soc., Mon. Not.*, 61, p. 10-12, 1900.

Armagh Observatory. Report for 1900. *Roy. Astr. Soc., Month. Not.*, 61, p. 219-220, 1901.

Further corrections to the Armagh catalogue, with special reference to the " anonymous " stars. *Roy. Astr. Soc., Month. Not.*, 61, p. 360-368, 1901.

TYCHO BRAHE's observatory. *Nature*, 65, p. 104-106, 1901.

TYCHO BRAHE's Fortjenester af Astronomien. (The astronomical merits of TYCHO BRAHE). Kjobenhaven, (36 p.), 1901.

The new star of 1572. *Observatory*, 24, p. 166, 1901.

Horrebow and sun-spots. *Observatory*, 26, p. 461-462, 1903.

A survey of the spiral nebula Messier 33 by means of photographs taken by Dr. ISAAC ROBERTS, F.R.S. *Roy. Irish Acad., Proc.*, 25 (Section A), p. 3-30, 1904, with 2 plates.

HEVELIUS and cometary orbits. *Observatory*, 28, p. 292, 1905.

History of the planetary systems from THALES to KEPLER. Cambridge, University press, (432 p.), 1906.

SOSIGENES. *Observatory*, 29, p. 256, 1906.

The comet of 1006. *Observatory*, 30, p. 248-249, 1907.

The eclipse of 1030. *Observatory*, 30, p. 354-355, 1907.

RALPH COPELAND. Nekrolog. *Astr. Gesell. Viertaljahrsschrift*, 42, p. 230-238, 1907, mit 1 Portr.

Second Index Catalogue of nebulae and clusters of stars; containing objects found in the years 1895 to 1907, with notes and corrections to the New General Catalogue and to the Index Catalogue for 1888-1894. *Roy. Astr. Soc., Mem.*, 59, (Part 2), p. 105-198, 1908.

Tercentenary of the telescope. *Nature*, 82, p. 190-191, 1909.

NEWTON or GALILEO? *Observatory*, 33, p. 250-251, 1910.

Observations of comets c 1908 snd a 1910 at Armagh Observatory. *Roy. Astr. Soc., Mon. Not.*, 70, p. 540-541, 1910.

Note on TYCHO BRAHE's opinion about the solar parallax. *Roy. Astr. Soc., Month. Not.*, 71, p. 74-76 with pl., 1911.

Origin of the « Astronomische Nachrichten ». *Observatory*, 34, p. 162, 1911.

WILLIAM HERSCHEL and JOHN MICHELL. *Observatory*, 34, p. 373, 1911.

Scientific papers of Sir WILLIAM HERSCHEL, knt. including early papers hitherto unpublished; collected and edited under the direction of a joint committee of the Royal Society and the Royal Astronomical Society, with a biographical introduction compiled mainly from unpublished material by DREYER. London, Royal Society, (2 vols.), 1912.

Summary of observations of the Solar eclipse of 1912, April 16-17. *Roy. Astr. Soc., Month. Not.*, 72, p. 542-545, 1912.

TYCHONIS BRAHE Dani Opera omnia, edidit I.L.E. DREYER. 14 vols. Hauniae, in Libraria Gyldendaliana, 1913-1928.

ARISTARCHUS. *Observatory*, 36, p. 303-307, 1913.

Corrections to the New General Catalogue, resulting from the revision of Sir WILLIAM HERSCHEL's three catalogues of nebulae. *Roy. Astr. Soc., Month. Not.*, 73, p. 37-40, 1912.

The well of ERATOSTHENES. *Observatory*, 37, p. 352-353, 1914.

Lord ROSSE's six foot reflector. *Observatory*, 37, p. 399-402, 1914.

The partial eclipse of the sun, August 20-21, 1914, observed at Armagh. *Roy. Astr. Soc., Month. Not.*, 75, p. 26, 1914.

Observations of the transit of Mercury, November 6-7, 1914, observed at Armagh. *Roy. Astr. Soc., Month. Not.*, 75, p. 37, 1914.

On TYCHO BRAHE's manual of trigonometry. *Observatory*, 39, p. 127-131, 1916.

On TYCHO BRAHE's catalogue of stars. *Observatory*, 40, p. 229-233, 1917.

WILLIAM HERSCHEL's letters. *Observatory*, 40, p. 307, 1917.

Equinoctial colure. *Observatory*, 40, p. 364-365, 1917.

On the origin of PTOLEMY's catalogue of stars. *Roy. Astr. Soc., Month. Not.*, 77, p. 528-39, 1917; 78, p. 343-49, 1918.

Descriptive catalogue of a collection of WILLIAM HERSCHEL papers presented to the Royal Astronomical Society by the late Sir W. J. HERSCHEL, Bart. *Roy. Astr. Soc., Month. Not.*, 78, p. 547-554, 1918.

WILLIAM HERSCHEL's observations of variable stars and stars suspected of variability. (Note following signed by J. L. E. DREYER.) *Roy. Astr. Soc., Month. Not.*, 78, p. 554-568, 1918.

KAISER as receiver of Chinese loot. *Observatory*, 41, p. 217, 1918.

Discussion of a meeting of the Royal Astronomical Society. *Observatory*, 42, p. 65-67, 1919.

Alleged new star of the year 1578. *Observatory*, 42, p. 161-162, 1919.

Equinoctial colure. *Observatory*, 42, p. 376, 1919.

The place of TYCHO BRAHE in the history of astronomy. *Scientia*, 25, p. 177-185, 1919. (Reviewed in *Nature*, 103, p. 134, 1919.)

Indian astronomical instruments. (A historical review.) *Nature*, 103, p. 166-168, 1919.

The tercentenary of JEAN PICARD. *Nature*, 106, p. 350-351, 1920.

TYCHO BRAHE's samlede skrifter. *Nordisk Astr. Tidsskrift*, NR 1, p. 69-74, 1920.

On the original form of the Alfonsine tables. *Roy. Astr. Soc., Month. Not.*, 80, p. 243-262, 1920.

Primitive chronology. *Nature*, 107, p. 274-276, 1921.

The cosmology of DANTE. *Nature*, 107, p. 428-430, 1921.

Mediaeval astronomy. Studies in the history and method of science, C. SINGER, ed. Oxford, 2, p. 102-120, 1921.

FLAMSTEED's letters to RICHARD TOWNELEY. *Observatory*, 45, p. 280-294, 1922.

Centenary of the death of WILLIAM HERSCHEL. *Nature*, 110, p. 255-256, 1922.

DOUBLET's " Histoire de l'astronomie." (Review.) *Nature*, 110, p. 600, 1922.

Address on the history of the Royal Astronomical Society, delivered on the occasion of the centenary celebration. *Roy. Astr. Soc., Month. Not.*, 82 p. 438, 1922.

History of the Royal Astronomical Society, 1820-1920, edited by J. L. E. DREYER, H. H. TURNER and R. A. SAMPSON, E. H. GROVE-HILLS, H. F. NEWALL and H. P. HOLLIS. London, Royal Astronomical Society, (258 p.) 1923.

Dr. ARTHUR A. RAMBAUT, F.R.S. (Obituary note). *Nat.* 112, p. 628-629, 1923.

Addresses delivered by the President on presenting the Jackson-Gwilt Medal and Gift to Mr. A. STANLEY WILLIAMS for his work on planets and variable stars, and to Mr. W. S. FRANKS for his work in star colours and observations on double-stars. *Royal Astr. Soc. Month, Not.*, 83, p. 428-30, 1923.

Address on the desirability of publishing a new edition of ISAAC NEWTON's collected works. *Roy. Astr. Soc., Month. Not.*, 84, p. 298-304, 1924.

Address delivered by the President in presenting the Gold Medal of the Society to Professor A. S. EDDINGTON, F.R.S. *Roy. Astr. Soc., Month. Not.*, 84, p. 548-558, 1924.

Early astronomy in Oxford. (Review of GUNTHER's " Early science in Oxford," 1923). *Nature*, 113, p. 38-40, 1924.

Greenwich Observatory, 1675-1925. *Nineteenth Century and After*, 98, p. 111-118, 1925. (Reviewed in *Nature*, 116, p. 59, 1925.)

Review of J. G. HAGEN's " First note on W. HERSCHEL's observations of extended nebulous fields". *Roy. Astr. Soc., Month. Not.*, 86, p. 146, 1926.

Letters from NEWTON, in Corpus Christi College, Oxford. ISAAC NEWTON, 1642-1727. Memorial volume edited for the Mathematical Association by W. J. GREENSTREET. London, BELL and sons. 181 p. 1927.

Biographies and obituary notices of
JOHN L. E. DREYER

Address delivered by the President, Prof. A. R. SAMPSON, on presenting the Gold Medal of the Royal Astronomical Society to Dr. J. L. E. DREYER. *Roy. Astr. Soc., Month. Not.*, 76, p. 368-375, 1916.

Dr. J. L. E. DREYER. Obituary notice. *Astr. Soc. Pacific, Publ.* 38, p. 400-401, 1926.

Dr. J. L. E. DREYER. Obituary notice, with portrait. *Observatory*, 49, p. 293-294, 1926.

Dr. J. L. E. DREYER. Obituary notice. *Nature*, 118, p. 454, 1926.

J. L. E. DREYER. Obituary notice. (Portrait). *Nord. Astr. Tids.*, 7, p. 116, 1926.

Dr. J. L. E. DREYER. Obituary notice, by E. B. KNOBEL. *Roy. Astr. Soc., Month. Not.*, 87, p. 251-257, 1927.

J. L. E. DREYER. Obituary notice, by CARL BURRAU. *Populär Astron. Tidskrift*, parts 3 and 4, 1926. Translated by PHELVA JERLOW for *Popular Astronomy*, 35, p. 325-327, 1927.

Dr. J. L. E. DREYER. Obituary notice, with portrait and bibliography, by N. E. NORLUND. *Danske Vidensk. Selsk.*. (p. 51-62) *Oversigt Forhandlinger*, 1926-27.

Reviews of DREYER'*s edition of* TYCHO BRAHE'*s works*

Tychonis Brahe Dani Opera omnia, edidit I. L. E. DREYER... Hauniae, in Libraria Gyldendaliana, 1913-1928. 14 vols. Illus., ports., tables, diagrs. 30 cm. " Auspiciis Societatis linguae et litterarum danicarum (Det Danske sprog- og litteratur-selskab)." " Numerus exemplorum venalium : 225."

Review by GEORGE SARTON of the first six vols. *Isis*, 5, p. 149-51, 1923.

Review by J. K. F. Tome X, *Nature* 114, p. 151-2, 1924.

Review by R. A. S. *Observatory* 49, p. 159-62, 1926.

Review by J. F. F. Tomes VIII, XII, and XIII, *Nature* 120, p. 578-79, 1927.

Review by J. K. F. Tomes IX and XIV, *Nature* 122, p. 803-04, 1928.

Reviews in *Astronomische Nachrichten*, Lit. Beibl. Nr. 60 (1925).

This bibliography forms a supplement to the life sketch of Dr. JOHN L. E. DREYER, the most distinguished historian of astronomy in recent times. An attempt has been made to include all his known published writings, both in scientific and historical research. A few items of a single-line note of observations were not included, as well as references wherein he was quoted.

All entries, such as books, pamphlets, articles, and extended notes, were arranged chronologically.

The principal bibliographical sources of DREYER's writings are to be found in the Catalogue of Scientific Papers of the Royal Society of London, vol. VII (1864-1873), vol. IX (1874-1883), vol. XIV (1884-1900), and in each of the succeeding volumes of the International Catalogue of Scientific Literature, series E (Astronomy), 1901-1914. From 1914 to 1926 a search was made in the various volumes of astronomical journals.

Library of Congress FREDERICK E. BRASCH.
Washington, D. C.

Check-list of Rotographs in the History of Natural and Occult Science

It may be of some service to students of the history of science and error, especially those in this country, to publish a summary list of rotographs now in my possession which will eventually be deposited in the Columbia University library. The rotographs are mainly of manuscripts in European libraries, and the majority of these of works of the fourteenth and fifteenth centuries, but there are also some from incunabula or other early modern printed works. A scholar desiring to examine one of the rotographs can arrange to do so at the Columbia University library or perhaps even to have the rotograph sent to his address for a brief period. Many of the treatises in question will be discussed in a forthcoming work continuing my researches in the history of magic and experimental science through the fourteenth and fifteenth centuries. Others as indicated have either been edited in whole or part or discussed in books and articles already published. There follow two alphabetical lists : the first by authors, the second by cities and libraries for the manuscripts used. The following abbreviations may require explanation :

BL	Bodleian Library
BM	British Museum
BN	Bibliothèque Nationale
CLM	Codex latinus Monacensis
DWS	Dorothea Waley Singer, *Catalogue of Latin and Vernacular Alchemical Manuscripts in Great Britain and Ireland dating from before the XVI century*, 1928-1931, 3 vols.
S. Marco	Bibliotheca Marciana

AUTHOR	TITLE, SHELF-MARK OF MS, ETC.	NUMBER OF ROTOGRAPH
Adam, Jehan	An Arithmetic composed 1475 A.D., closing pages. Paris, Ste. Geneviève français 3143, fols. 66 v°-75 r° : described, *Science and Thought in the Fifteenth Century*, 1929, Chapter IX; its table of contents printed Ibid., pp. 302-305.	10
Albandinus	Work of astrological medicine, opening, "Cum secundum varietates temporum..." : Munich, CLM 527, fols. 36r-41r.	151
Albertus Magnus	Commentary on the *De secretis mulierum* of: Utrecht 723, 1353 A.D., fols. 46 r°-78 r°.	96
Alhandreus	Opening, "Luna est frigide nature..." Paris, BN 17868, 10th century, fols. 2 r°-12 v°.	19
	London, BM Additional 17808, 11th century, fols. 85 v°-99 v°.	23
	Munich, CLM 560, fols. 61 r°-87 v°. Described in *Magic and Experimental Science*, 1923, Chapter XXX.	12
	Opening, "Igitur quoniam in hac arte utilissimum..." : Rome, Vatican 4084, fols. 1 r°-14 v°.	14
Andalò di Negro	Canons on the Almanach of Profatius Judaeus : Paris, BN 7272, fols. 68 r°-84 v°	89
	De iudiciis infirmitatum : Rome, Vatic. 4082, fols. 196 r°-209 r°.	88 A
	Introductorium ad iudicia astrologie : Paris, BN 7272, fols. 101 r°-102 r°.	89
	Theorica planetarum, Table of Contents and Explicit only : Paris BN 7272, fols. 42 v°-43 v°.	89

Andreas de Sommaria On astrology : Rome, Vatic. 989, fols. 57 r°-69 v°. 13

Anonymous Compositum de conpositis (Paris, May, 1331) : Edinburgh, National library 20.8.1, 14th century, fols. 6 r°- 40 v°. DWS, No. 290; but the work is not by masters of Paris as there stated, but by someone writing in the first person singular. 126

De causis naturalibus quorundam eventuum futurorum : Tours 520, 15th century, fols. 97 r°-104 v°. 94

De effectibus lune : Erfurt, Amplon. F. 386, circa 1359 A.D., fols. 53 v°-55 v° : " Nota lunam debilitari..." 72

De naturis planetarum : Erfurt, Amplon. F. 386, c. 1359 A.D., fols. 55 v°-58 : " Et cupientes habere..." 72

De ovo philosophorum : Cambridge, Trinity 1122, fol. 38 r°. 118

De presagiis tempestatum : Oxford, Bodleian, Laud. Misc. 594, fols. 115 r°-116 v° : opening, " Purus oriens atque non fervens serenum..." 87

De septem artibus liberalibus : Berne 260, fol. 227. 147

De spera : Paris, BN 7272, fols. 60 r°-67 v°. 89

(Donum Dei) but here without title : Wolfenbüttel 3772 (77.2 Aug. 8°), 15th century, fols. 1-14, opening, " Qui desiderant artis philosophice scientie... ". 148

Expositio intentionis regis Alfonsi circa tabulas eius : Paris, BN 7281, fols. 156 v°-158 v°, but the rotograph is of the last page only. 65

Lumen luminum abbreviatum : Cambridge, Trinity 1122, fol. 128. 121

Notae astronomicae : Erfurt, Amplon. F. 386, c. 1359 A.D., fols. 48-49 v°, opening, " Philosophi compositis tabulis verum locum..." 72

Practica astrolabii : Paris BN 7272, fols. 45 r°-59 v°, " Incipit practica astrolabii et primo de nominibus partium instrumenti..." 89

Questiones multe et varie cum solutionibus de naturalibus rebus : Munich, CLM 19368, fols. 119-130, opening, "Ad sciendum solutiones...". 152

Septiformis de moralitatibus rerum nature : Munich, CLM 8809, 1426 A.D., fol. 2 r°-v° (opening only). 22

Theorica distantiarum omnium sperarum circulorum et planetarum a terra et de magnitudine eorum : Paris, BN 7272, fols. 84 r°-99 v°, opening, " Punctus centrum cuspis sunt sinonima..." 89

Anselm, see Giorgio Anselmi of Parma.

Arnald of Villanova De arte cognoscendi venena : from the printed copy of *Archana medicine*, Geneva, 1498, fols. 57 r°-58 v°, in the Surgeon General's library, Washington, D. C. 73

Flos lilii : Bologna, University library 138 (104), 15th century, fols. 126 r°-129 r°. 137

Rosarius : London, BM Sloane 2476, 15th century, fols. 48 v°-68 r°. In the MS and by DWS, No. 286, the work is ascribed to John Dastin under the title, Speculum philosophie. 129

Augustine of Trent Prediction for 1340 and pest tract : Munich, CLM 276, fols. 87 r°-91 r°; 3

CLM 647, fols. 1 r°-20 v°. Printed in part in *Sudhoffs Archiv f. Geschichte d. Medizin*, XXIII (1930), 346-356. 49

Baldus de Tambenis of Faenza, John Opuscula, including De temporibus partus : Florence, Laurent. Plut. 19, cod. 30, 15th century, 45 fols. See *Science and Thought in the Fifteenth Century*, 1929, pp. 25-28. 18

Bartholomaeus Vienna 2520, fols. 50 v°-68 r° : "Incipiunt dicta magistri Bertholomei... Circa artem pronosticationis quatuor sunt notanda..." 42

Bartoli vita Rome, Vatic. 7031, fols. 179 r°-180 v°, 195 r°-196 v°. 34

Bernard Tornius, see Tornius.

Bernard of Verdun Speculum celeste, astronomical in ten tractates : Erfurt, Amplon. F. 386, c. 1359 A.D., fols. 1-25. 72

Bertipaglia, see Leonard of.

Bistichius Medical recipes : Venice, S. Marco VI, 282, fols. 77 r°-80 r° : reproduced in part in *Science and Thought in the Fifteenth Century*, 1929, pp. 265-268. 16

Blasius of Parma Questions on the Perspectiva communis of John Peckham : Vienna 5309, fols. 67 r°, 117, 126 v°; Vienna 5447, fols. 1 r°, 24 v°-25 r°, 115 v°, 131 r° (Extracts only). 141

Boissard, J. J. *De divinatione et magicis praestigiis*, 1616, pp. 49-50, de magia, from printed copy at Paris, BN R 744. 107

Bonincontri, Laurentius (Miniatensis) Commentaria in suos libros rerum naturalium et divinarum : Rome, Vatic. 2845, fol. 66 (Extract only). 33

Braccia, see John Braccia or Ptraccia.

Broun, Thomas Commentaries on constellations of 1425 and 1435 : Oxford, BL Digby 194, fols. 95 v°-99 r°. 57

Buitzruss, Conrad Personal memoranda : Munich, CLM 671, fol. 1. printed *Speculum*, IV (1929), 88-89. 11

Calculator, see Suiseth, Richard.

Calderia, Ioannes (of Venice) Liber canonum astrologiae : Venice, S. Marco XX, 95, fols. 41 r°-129. 52

Campanus Practica de dispositione aeris : London, BM Sloane 332, fol. 16. 70

Candidus, see Decembrio, Pier' Candido.

Christopher of Paris Cithara : Pavia, University library 341, fols. 22 r°-23 v°. 110

Conrad Heingarter, or Thuricensis, see Heingarter.

D'Ailly, see Pierre.

Daniel Morley De creatione mundi : Cambridge, University library 1935 (Kk.I.1), 13th century, fols. 98 r°-v°, 100 r°, 105 r° (Extracts only): see *Magic and Experimental Science*, II, 171, note. 1

Danko, see John of Saxony.

Decembrio, Pier' Candido De genitura hominis, from *Archana medicine*, Geneva, 1498, printed copy, Surgeon General's library, Washington, D.C. 73

Dastin, John De arte alchemica : London, BM Sloane 2476, 15th century, fol. 3 : DWS, No. 287. 129

Epistola ad Iohannem XXII : Cambridge, Trinity 1122, fols. 94 v°-95 v° : " Hoc est secretum..." DWS, No. 280. 120

Libellus aureus : Cambridge, Trinity 1122, fols. 36 v°-38 r° : " Testatur ad credendum..." 118

Oxford, BL Fairfax 22, fols. 29 r°-30 v°. DWS, No. 285. 115

	Liber philosophie : London, BM Stowe 1070, fols., 44-51. DWS, No. 281.	131
	BM Sloane 2476, fols. 4-6, 40-68.	129
	Rosarius opening, " Desiderabile desiderium..." Cambridge, Trinity 1122, fols. 81 r°-94 r° : DWS, No. 231.	119
	Manchester, John Rylands 65, fols. 55 r°-73 r°.	9
	Rosarius opening, " Sciendum itaque quod lapis..." London, BM Harl. 3528, fols. 171-175 : DWS, No. 283.	132
	Oxford, BL Ashmole 1416, fols. 119 r°-122 v°.	134
	Verbum abbreviatum : Oxford, BL Ashmole 1416, fols. 73 r°-87 r°, 88 v°-93 r° (fols. 87 v°-88 r° and 93 v° are missing) : DWS, No. 284.	127
	Visio : DWS, No. 282 : London, BM Sloane 2476, fols. 1-2 v°.	129
	London, BM Harl. 1747, fols. 43 r°-45 v°.	130
Edward	Visio, DWS, No. 334 : Oxford, BL Fairfax 22, fols. 1-2.	113
	Cambridge, Trinity 1122, fols. 126 v°-128 r°.	121
Egidius (O. P.)	De essentia, motu et significatione cometarum: Oxford, BL Ashmole 341, 13th century, fols. 61 r°-76 r°.	86
Engelbert, abbot of Admont	De fascinatione : Claustroneoburgi 306, 14th century, fols. 164 v°-179 r° : " Cum secundum philosophum in primo Posteriorum..."	144
Entisberi, see William of Heytesbury.		
Eschenden, see John of.		
Evno of Würzburg	Iudicia de impressionibus in aere : Norimberg cent. V, 64, Heft	

1, 15th century, fols. 92v-102r, opening, "Felix inquam nimium prior aetas...". 153

Exafrenon, see Richard of Wallingford.

Fagarola, Petrus Medical advice to his two sons at Toulouse : London, BM Sloane 3124, fols. 74 r°-77 r°; printed, *Speculum*, VI (1931), 110-114. 47

Firminus de Bellavalle De mutatione aeris, opening, "Quia in multis voluminibus sapientes..." : Paris, BN 7482, fols. 34 r°-35 r°, 69 v°-71 r°, 156. 69

Rome, Vatic. Palat. 1340, fols. 209 v°-242 r°. 78

Pronosticatio super coniunctione Saturni et Iovis et Martis A.D. 1345, opening, "Tres principes de militia superiorum..." : Paris, BN 15104, fols. 206 r°-206 v°; 76

Erfurt, Amplon. F. 386, c. 1359 A.D., fols. 59 v°-60. 72

and John of Murs, Epistola super reformatione antiqui kalendarii : Paris, BN 15104, fols. 208 v°-216 r°. 76

Fontana, Giovanni da Metrologum de pisce, cane et volucre : Bologna, University library 1426 (2705), 15th century, fols. 85 r°-105 v°.

Gaufridus de Meldis, see Geoffrey of Meaux.

Gawir, see Nicolaus de.

Geber Summa perfecti magisterii, opening, "Istam veram scientiam quam ex libris..." : Cambridge, Corpus Christi 99, 15th century, p. 36 (opening page only). 117

Gentile da Foligno De demonibus, Wolfenbüttel 2794 (81.4. Aug. 2°), fols. 290 r°-291 r°. 71

De incubo, Ibid., fols. 279 v°-281 v°. 71

Geoffrey of Meaux Astrological medicine, opening,

"Totius astronomie iudicialis compendium..." : Avignon 1022, fols. 203 v°-206 r°. 56

London, BM Sloane 1680, fols. 42 r°-45 v°. 45

On the comet of 1315, opening, "Omnibus in stellarum forma studentibus..." : London, BM Sloane 1680, fols. 45 v°-46 v°. 45

On the comet of 1337, opening, "Ad honorem illius sanctissimi astronomi..." : Erfurt, Amplon. F. 386, c. 1359 A.D., fols. 55 v°-58. 72

George of Trebizond De antisciis tractatus et cur his temporibus astrologorum iudicia fallunt: Rome, Vatic. Reg. Suev. 1098, fols. 1 r°-15 v°. 139

Giorgio Anselmi of Parma Theoremata radicalia : Rome, Vatic. 4080, fols. 41 r°-53 r°. 36

Quarta pars quarti tractatus de modis specialibus imaginum octavi orbis : Rome, Vatic. 5333, 1542 A.D., fols. 1 r°-38 v°. 50

De magia disciplina : Florence, Laurentian Plut. 45, cod. 35, fols. 1-20, 221-231. 154

Gerardus de Silteo vel de Sileto Summa de astris : Bologna, Bibl. Comunale A. 539, fols. 1 r°-95 v°. 75

Giovanni da Fontana, see Fontana : for other Giovanni's see John.

Gregory, Gaspard de Tavola cronologica di tutti gli scrittori ed artisti, 1400-1500, from his *Storia della Vercellese litteratura*, Turin, 1819-1824, pp. 514-533 : Paris, BN Z 8983-8986 (printed book). 54

Guaynerius (Antonio Guaineri) De venenis, table of contents : Rome, Vatic. 8759, fols. 1 r°-4 v°. 140

Haly — De proprietatibus lune, opening, "Luna si fuerit in prima domo significat..." : Erfurt, Amplon. F. 386, fol. 52. 72

Heingarter, Conrad — Commentary on Ptolemy's Quadripartitum : Paris, BN 7305, fols. 4 r°-10 r°; 82

Paris, BN 7432, fols. 1 r°-10 r°. 83

Nativity of Jehan de la Gutte : Paris, BN 7446, fols. 1 r°-14 r°. 109

Tractatus de cometis (printed) : London, BM IB 38108, fols. 1 r°-13. 85

Henry, physician to king of Bohemia — Fictio mercurii : Oxford, BL Digby 43, 14th century, fol. 120 v°. 125

Henry of Hesse — Contra coniunctionistas : London, BM Sloane 2156, fols. 209 v°-224 r°; 39

Florence, Laurentian, Ashburnham 210, fols. 71 r°-84 v°. 41

De habitudine causarum et influxu nature communis respectu inferiorum : London, BM Sloane 2156, fols. 194 v°-209 v°; 39

Rome, Vatic. 3088, fols. 14 r°-28v° ; 36

Vienna 4217 (Theol. N. 608), fols. 1 r°-9 v°. 37

De reductione effectuum particularium in causas universales : London, BM Sloane 2156, fols. 116 v°- 130 v°; 39

Florence, Laurent. Ashburnham 210, fols. 89 r°-101 r°; 41

Vienna 4217, fols. 29 r°-38 r°. 37

Epistola... de prophetia Hildegardis : Wolfenbüttel 270 (237 Helmst.), fols. 385 r°-392 r°; 105a

Wolfenbüttel 402 (367 Helmst.), fols. 31 v°-38 r°. 105b

Questio de cometa (of 1368) : Vienna 4217, fols. 38 v°-45 r°. 37

	Queritur utrum secundum naturalem philosophiam sint alique substantie separate preter motores orbium celestium : Rome, Vatic. 9369, fols. 26 r°-40 r°.	43
	Tractatus de discretione spirituum : Rome, Vatic. 9369, fols. 1 r°-15 r°.	43
Hermann (?)	Practica tabularum Alfonsi : Erfurt, Amplon. F. 386, fols. 38-48.	72
Hermes	Centiloquium : Erfurt, Amplon. F. 386, fol. 50.	72
Hippocrates (Pseudo-)	Astronomia : Erfurt, Amplon. F. 386, fols. 52 v°-53 v°.	72
Heytesbury, see William of.		
Hitlisbyry, see William of Heytesbury.		
Hugh of Siena	De modo generationis mixtorum ex elementis questio Bononie disputata : Venice, S. Marco XII, 20, 1439 A.D., fols. 153 r°-155 r°;	51
	Questio de augmentatione : Ibid., fols. 113 r°-116 r°.	51
Icocedron philosophie, see Walter of Odington.		
Jean, see John.		
Jehan, see Adam, Jehan.		
John XXII	Bull, " Spondent quas non exhibent " (against alchemists) : London, BM Royal 7.E.X, fol. 47 v°.	47
John of Arezzo	De procuratione cordis : Florence, Laurent. Plut. 73, cod. 29, six unnumbered pages containing Tractatus iii, cap. 15, " De notitia ac signis animalium rabiosorum."	33
John Baldus de	Tambenis de Faventia, see Baldus.	
John Bombeles	Stella alchimie, opening, " Cum omnium philosophorum documenta..." : Wolfenbüttel 3282 (23.17. Aug. 4°), 1497 A.D., 224 v°-244 r°.	104
John Braccia (?)	Practica cirurgie : see *Science and Thought in the Fifteenth Century*,	

1929, Chapter IV : Munich, CLM 273, 1453 A.D., fols. 113-146; 24
Munich, CLM 321, 15th century, fols. 207-249; 4
Vienna 2358, 1436 A.D., fols. 124 r°-149 v°; 6
Vienna 4751, 1424 A.D., 5 v°-212 r°. 7

John Calderia Venetus, see Calderia.

John Dastin, see Dastin.

John of Eschenden Summa iudicialis, extracts from : Munich, CLM 221, fols. 155 r°-176 v°; 46
Oxford, BL Bodley 714, fols. 176 r°-177 v°; 59
Oxford, Oriel 23, fols. 169 r°-170 v°. 58
De tribus coniunctionibus (a prediction for 1368-1374) : Oxford, BL Ashmole 393. I, fols. 79 r°-80 r°; 79
Ashmole 192. IV, fols. 12 r°-16 v°. 80

John de Fundis, of Bologna Nova theorica planetarum and Nova spera materialis: Utrecht 724, fols. 56 r°-63 r°, 63 v°-68 v°. 145
Questio de durationis huius etatis mundi : London, BM Royal 8.E.VII, fols. 73 r°-77 v°. 146

John of Gmunden Calendar : Munich, CLM 7650, fols. 1 r°-8 r°. 135*a*
Astronomical tables and instructions, opening, " Proprietates signorum secundum magistrum Iohannem de Gmunda " : Munich, CLM 8950, fols. 81 r°-93 v°. 135*b*

John of Imola Utrum scientia civilis vel canonica sit nobilior medicinali : Venice, S. Marco VI, 105, fols. 79 v°- 82 r°. See *Science and Thought in the Fifteenth Century*, 1929, pp. 261-262. 15

John of Itri (Johannes Itrensis) De peste et epydimia, from *Archana medicine*, Geneva,

1498, fols. 66 r°-71 v°, printed work at Washington, Surgeon General's library. 73

John Lauratius de Fundis, see John de Fundis.

John of Legnano On the comet of 1368 : Rome, Vatic. 989, fols. 70 r°-81 r°; 13
Vatic. 2369, fols. 212 r°-214 v°. 92

John Marlianus, see Marliani.

John de Murs Treatise of 1317 and Calendar, opening, "Autores kalendarii nostri duo..." : Vienna 5292, 15th century, fols. 199 r°-209 v°. 77
Kalendarium et Patefit : Erfurt, Amplon. Q. 360, 1360 A.D., fols. 33 v°-55 v°; 142
Amplon, Q. 371, 14th century, fols. 8 r°-45 v°. 143
Canones tabularum Alfonsi compositi Parisius in Sorbona : Oxford, Hertford 4, fols. 140 r°-147 r°. 101
Pronosticatio super coniunctione (of 1345 A.D.), opening, "Ex doctrina mirabili sapientum..." : Erfurt, Amplon. F. 386, fols. 60-61 v°; 72
Paris, BN 15104, fols. 206 v°-208 r°. 76
Epistola ad Clementem VI on the conjunction of 1365 : Paris, BN 7443, fols. 33 r°-34 v°. 67

and see Firminus.

John Paul de Fundis, see John de Fundis.

John Peckham (Pisanus), see Blasius of Parma.

John Philippus de Lignamine Messanensis Siculus Conservatorium sanitatis, from *Archana medicine*, Geneva, 1498, printed copy in Surgeon General's library, Washington. 73

John de Probavilla On prognostic signs : Vienna 2520, fols. 37 r°-50 v°. 42

John de Protsschida Compendium de ocultis nature :

Munich, CLM 27006, fols. 216 r°-231 r°. 38

John of Rupescissa Liber lucis, opening, " Consideravi tribulationes electorum..." : Copenhagen Gl. kgl. S. 1712, 15th century, fols. 111-118 v°; 90

Copenhagen Gl. kgl. S. 236, fols. 52 r°-54 v°; 91

Klagenfurt, Bischöfl. Bibl. XXIX. d. 24, 1421-1423 A.D., fols. 202 v°-214 r°; 95

Oxford, BL Ashmole 1423, V, pp. 68-77; 133

Venice, S. Marco fondo antico 323, fols. 114 v°-119 r°. 93

Visions and prophecies (under 30 heads) : Paris, BN 3598, fols. 1 r°-46 r°. 106

John of Saxony Almanach, opening, " Cum animadverterem quamplurimos : Erfurt, Amplon. F. 368, fols. 62-108. 72

Declaratio theorice planetarum : Ibid., fols. 34 r°-38 r°. 72

Expositiones canonum primi mobilis (of John de Lineriis) : Ibid., fols. 26-32. 72

John Tolhopf, see Tolhopf.

John Tyrus Anglicus, see Dastin.

John of Vienna Speculum elementorum : Oxford, BL Fairfax 22, fols. 13 r°-17 v°. See DWS, No. 304. 114

Lambourne, Reginald Epistola de coniunctionibus : Oxford, BL Digby 176, fols. 40-41 v°. 81

Leonard of Bertipaglia Cirurgia, extracts from : Leipzig, Institut für Geschichte der Medizin A. 1, fols. 194 v°-198 v°. 8

Cirurgia, table of contents : London, BM Sloane 3863, fols. 109 r°-110 r°. *1a*

Lilium, see Paridis.

Lull, Raymond (Pseudo-) Anima artis transmutationis, opening, "Fulgeat regis dyadema..." : Oxford, BL Bodley 645, fols. 74 r°-96 v°. DWS, No. 253. 111
Aurum potabile : Cambridge, Corpus Christi 99, fols. 106-108. DWS, No. 265. 117
"Iste est modus de lapide minerali..." : Oxford, Corpus Christi 136, fols. 1 r°-7 v°. DWS, No. 263. 112
Bodleian Fairfax 22, fols. 26-27. 149
De secretis nature, opening, "Contristatus erat Raymundus..." : Oxford, BL Bodley 645, fols. 1-72. DWS, No. 255. 111
With another incipit : Milan, Ambros. D. inf. 512, fols. 1 r°-34 r°. 97
Vade mecum (or, Codicillus) : Copenhagen Gl. kgl. S. 236, fols. 57 r°-83 r°. 99
Maino de Maineri De saporibus : Naples, Bibl. Naz. VIII. D. 35, fols. 52-53. 155
Marliani, Giovanni De reactione : Venice, S. Marco VI. 105, fols. 1 r°-47 r°. 26
Morley, see Daniel.
Niccolò da Foligno De ideis : Rome, Vatic. 3897, fols. 79 r°-86 r°. 2
Another version for Lorenzo de' Medici : Florence, Laurent. Plut. 82, cod. 22, fols. 1 r°-47 r°. See *Science and Thought in the Fifteenth Century*, 1929, Chapter X, and Appendix 18 (pp. 308-331) where the first version is printed.
Nicolaus Comes (or, de Comitibus) Speculum alchimiae, opening, "Ut ad perfectam scientiam..." DWS, No. 355 : London, BM Sloane 692, fols. 20 r°-46 v°; 102
Stowe 1070, fols. 2 r°-17 r°; 103

Cambridge, Corpus Christi 99, pp. 21-35. 117

Nicolaus de Gawir (Nikolaus Jauer) De superstitionibus, Table of contents : London, BM Harl. 3767, fols. 89 v°, 91 r°. 1*b*

Oresme, Nicolas Tract against astrology, opening, "Multi principes et magnates..." : Florence, Laurentian, Ashburnham 210, fols. 84 v°-89 r°; 41
Vienna 4948, fols. 162 r°-168 r°; 27
Rome, Vatic. 4275, fols. 35 r°-40 r°. 31
Contra divinatores horoscopios : Florence, Laurentian, Ashburnham 210, fols. 3 r°-21 r°; 41
Paris, BN 15126, fols. 1 r°-39 r°; 35
De commensurabilitate motuum celestium : Rome, Vatic. 4082, fols. 97 r°-108 v°. 31
De configuratione qualitatum : Paris, BN 14579, fols. 33 r°-36 r°; 14580, fols. 52 r°-57 r°. 29
De divinationibus : Oxford, BL Canon. Misc. 248, fols. 28 r°-33 v°. 32
Quotlibeta : Florence, Laurentian, Ashburnham 210, fols. 21 v°-70 v°. 55
Optime questiones super perspective (authorship doubtful) : Erfurt, Amplon. F. 380, 14th century, fols. 29 r°-40 v°.

Ortolanus, Martin (?) Commentary on the Emerald Tablet of Hermes : Copenhagen Gl. kgl. S. 237, fols. 18 v°-22 r°; 98
Paris, BN 11201, fols. 84 r°-96 r°, with another alchemical tract at fols. 76 r°-83 r°. 100

Paridis Lilium : Cambridge, Corpus Christi 99, p. 109, giving only the opening of the treatise 117

Perscrutator (Robert of York?) De impressionibus

aeris : Cambridge, University library Ii.I.1, fols. 13 r°-25 r°; 64
Munich, CLM 275, fols. 144 r°-154 v°. 44

Peter of Abano De venenis, in *Archana medicine*, Geneva, 1498, printed copy in Surgeon General's library, Washington. 73

Petrus Fagarola, see Fagarola.

Petrus de Zelence (Silento ?) De occultis : Manchester, John Rylands 65, fols. 146 r°-151 v°. 9

Pierre d'Ailly Apologia defensiva astronomiae ad Iohannem Gersonium : Paris, BN 2692, fols. 145 r°-147 v°. 108

Probavilla, see John de.

Protsschida, see John de.

Ptraccia, see John Braccia.

Quiricus de Augustis de Terdona *Lumen apothecariorum*, Turin, 15 Feb. 1492 : from printed copy in Surgeon General's library, Washington. 74

Rasis Centiloquium : Erfurt, Amplon. F. 386, fols. 50 v°-52. 72

Raziel Liber secretorum, Incipit prologus super tractatum sextum qui dicitur Samayn : Munich, CLM 51, fols. 111-123v. 150

Reginald, see Lambourne.

Rhazes, see Rasis.

Richard of Wallingford (?) Exafrenon pronosticationum temporis, Oxford, BL Digby 194, fol. 95 v° : the last leaf of the work only 57

Robert of York, see Perscrutator.

Romanus, magister De significatione stellarum (or, De xvi impedimentis in astronomia) : Munich, CLM 25005, fols. 50 r°-68 r°; 49
Erfurt, Amplon. Q. 368, 14th century, fols. 63 r°-70 v°. 60

Rosarius, see Arnald of Villanova and Dastin, John.

Sarne — Lilium alchimiae, opening, " Natura circa solem et lunam..." : Oxford, BL Ashmole 1384, 14th century, fols. 65 r°-74 v°. DWS, No. 335. 116

Silteo, see Gerard de.

Sommaria, see Andreas.

Suiseth, Richard — Liber calculationum, from edition of 1485, IB 29968 in British Museum, London. See *Speculum*, VII (1932), 221-230. 28

De motibus naturalibus : Erfurt, Amplon. F. 135, 1337 A.D., fols. 25 v°-47 r°. 123

Swynshede, Roger; see Suiseth, Richard.

Themo, magister — Anno gratie 1350 fuit questio disputata apud Scotos in Erfordia per magistrum Themonem : Erfurt, Amplon. F. 313, fols. 121-142 v°; 62

Amplon, F. 380, fols. 42-48. 63

Alia questio astronomica : Erfurt, Amplon. F. 313, fols. 159-165 v°. 61

Thomas of Bologna — Alchemical epistle to Bernard of Treves, with the first page of Bernard's reply : Paris, BN 11201, fols. 1 r°-14 r°. 100

Thurecensis, see Heingarter, Conrad.

Tideus — De qualitate eius quod videtur in speculo : Rome, Vatic. 2975, fols. 157 r°-159 v°. 30

Tiraquellus — Medicorum per alphabetum nomenclatura, from his *De nobilitate*, cap. 31, printed edition of 1571, fols. 121 r°-148 r°. 20

Tolhopf, John — De motibus celestium mobilium (to Sixtus IV) : Rome, Vatic. 3103, fols. 1 r°-32 r°. Discussed in *Science and Thought in the Fifteenth Century*, 1929, pp. 298-301. 25

Tornius, Bernard — Relatio anatomica : Florence, Ric-

	card. 930, fols. 17 v°-19 v°. Printed in *Science and Thought in the Fifteenth Century*, 1929, pp. 290-294.	5
	De tuenda sanitate, first part only : Florence, Laurent. Plut. 73, cod. 34, fols. 1-30.	40
Valascus de Taranta	De peste et epydimia, from *Archana Medicine*, Geneva, 1498, printed copy in Surgeon General's library, Washington.	73
Vemaldus	De operationibus et preparationibus veris (alchemical) : Manchester, John Rylands 65, fols. 158 v°-164 r°, DWS, No. 340.	9
Walter of Odington	Icocedron : Cambridge, Trinity 1122, 14th century, fols. 177 v°-183 v°;	122
	London, BM Addit. 15549, fols. 4 r°-22 v°. DWS, No. 289.	128
	see also Richard of Wallingford, Exafrenon, a work of which Walter is perhaps rather the author.	
William of Heytesbury	Primus tractatus de eventu futurorum : Venice, S. Marco X. 207, 14th century, fols. 78 r°-79 r°.	51

City, Library, Collection and Shelf-Mark	Leaves covered	Number of rotograph
Avignon 1022	203 v°-206 r°	56
Berne 260	227	147
Bologna Bibl. Comunale A. 539	1 r°-95 v°	75
University library 138 (104)	126 r°-129 r°	137
Univ. library 1426 (2705)	85 r°-105 v°	138
Cambridge Corpus Christi 99	pp. 21-36, 106-109	117
Trinity 1122	36 v°-38 r°	118
	81 r°-94 r°	119
	94 v°-95 v°	120
	125 v°-128 v°	121
	177 v°-183 v°	122

University library Ii.I.1	13 r°-25 r°	64
Univ. library Kk.I.1	98, 100 r°, 105 r°	1*a*
Claustroneoburgi 306	164 v°-179 r°	144
Copenhagen Gl. kgl. S. 236	52 r°-54 v°	91
	57 r°-83 r°	99
237	18 v°-22 r°	98
1712	111 r°-118 v°	90
Edinburgh National library 20.8.1	6 r°-40 v°	126
Erfurt Amplon. F. 135	25 v°-47 r°	123
F. 313	121-142 v°	62
	159-165 v°	61
F. 380	29-40 v°	124
	42-48	63
F. 386	1-108	72
Q. 360	33 v°-55 v°	142
Q. 368	63 r°-70 v°	60
Q. 371	8 r°-45 v°	143
Florence Laurentian Ashburnham 210	3 r°-21 r°,	41
	71 r°-101 r°	41
	21 v°-70 v°	55
Plut. 19, cod. 20	1 r°-45 r°	18
Plut. 45, cod. 35	1-20, 221-231	154
Plut. 73, cod. 29	six pp.	33*b*
Plut. 73, cod. 34	1-30	40
Plut. 82, cod. 22	1 r°-47 r°	17
Riccardian 930	17 v°-19 v°	5
Klagenfurt Bischöfl. Bibl. XXIX d 24	202 v°-214 r°	95
Klosterneuburg, see Claustroneoburgi.		
Leipzig Institut f. Gesch. d. Med. A. 1	194 v°-198 v°	8
London British Museum Additional		
15549	4 r°-22 v°	128
Addit. 17808	85 v°-99 v°	23
Harley 1747	43-45 v°	130
3528	171-175	132
3767	89 v°, 91 r°	1*b*
Royal 7 E X	47 v°	47*b*
8 E VII	73 r°-77 v°	146
Sloane 332	16 r°-v°	70
692	20 r°-46 v°	102

	1680	42 r°-46 v°	45
	2156	116 v°-130 v°, 194 v°-224 r°	39
	2476	1-6, 40-68	129
	2839	5 v°-8 r°	53
	3124	74 r°-77 r°	47*a*
	3863	109 r°-110 r°	1
Stowe	1070	2 r°-17 r°	103
		44-51	131
Manchester John Rylands library	65	55 r°-73 r°, 146 r°-151 v°, 158 v°-164 r°	9
Milan Ambros.	D. inf. 512	1 r°-30 r°	97
Munich CLM	51	111-123	150
	221	155 r°-176 v°	46
	273	113-146	24
	275	144 r°-154 v°	44
	276	87 r°-91 r°	3
	321	207 r°-249 v°	4
	527	36r-41r	151
	560	61 r°-87 v°	12
	647	1 r°-20 v°	49*a*
	671	1 r°-v°	11
	7650	1 r°-8 r°	135*a*
	8809	2 r°-v°	22
	8950	81 r°-93 v°	135*b*
	19368	119-130	152
	23888	59 r°-60 r°	49*b*
	25005	50 r°-68 r°	49*c*
	27006	216 r°-231 r°	38
Naples VIII.	D. 35	52-53	155
Nürnberg cent.	V, 64	92v-102r	153
Oxford Bodleian	Ashmole 192, IV	12 r°-16 v°	80
	341	61 r°-76 r°	86
	393, I	79 r°-80 r°	79
	1384	65 r°-74 v°	116
	1416	73 r°-87 r°, 88 v°-93 r°	127
		119 r°-122 v°	134
	1423, V	pp. 68-77	133

	Bodley 645	1-72, 74 r°-96 v°	111
	714	176 r°-177 v°	59
	Canon. Misc. 248	28 r°-33 v°	32
	Digby 43	120 v°	125
	176	40-41 v°	81
	194	95 v°-99	57
	Fairfax 22	1 r°-2 v°	113
		13 r°-17 v°	114
		26-27	149
		29 r°-30 v°	115
	Laud. Misc. 594	115 r°-116 v°	87
	Corpus Christi 136	1 r°-7 v°	112
	Hertford 4	140 r°-147 r°	101
	Oriel 23	169 r°-170 v°	58
Paris BN	2692	145 r°-147 v°	108
	3598	1 r°-46 r°	106
	7272	42 r°-102 r°	89
	7281	158	65
	7305	4 r°-10 r°	82
	7378 A	62 v°-64 r°	66
	7432	1 r°-10 r°	83
	7443	33 r°-34 v°	67
	7446	1 r°-14 r°	109
	7482	34 r°-35 r°, 69-71 r°, 156	69
	11201	1 r°-14 r°, 76 r°-102 r°	100
	14579	33 r°-36 r°	29*a*
	14580	52 r°-57 r°	29*b*
	15104	48-58 v°	68
		206 r°-216 r°	76
	15126	1 r°-39 r°	35
	17868	2 r°-12 v°	19
Ste. Geneviève	3143 français	66 v°-75 r°	10
Pavia University library	341	22 r°-23 v°	110
Rome Vatican	989	57 r°-81 r°	13
	2418	3 r°-8 r°	136
	2639	212 r°-214 v°	92
	2845	66 r°-v°	33*a*

	2975	157 r°-159 v°	30
	3088	14 r°-28 v°	36*a*
	3103	1 r°-32	25
	3897	79 r°-86 r°	2
	4080	41 r°-53 r°	36*b*
	4082	97 r°-108 v°	31*a*
		196 r°-209 r°	88*a*
	4084	1 r°-14 v°	14
	4275	35 r°-40 r°	31*b*
	5333	1 r°-38 v°	50
	7031	179 r°-180 v°,	
		195 r°-196 v°	34
	8759	1 r°-4 v°	140
	9369	1 r°-15 r°,	
		26 r°-40 r°	43
Vatican	Ottobonian 1870	107 r°-109 r°	88
	Palat. 1340	209 v°-242 r°	78
	Reg. Suev. 1098	1 r°-15 v°	139
Tours	520	97 r°-104 v°	94
Utrecht	723	46 r°-78 r°	96
	724	56 r°-68 v°	145
Venice S. Marco	VI. 105	1 r°-47 r°	26
		79 v°-82 r°	15
	VI. 282	77 r°-80 r°	16
	X. 207	78 r°-79 r°, 113 r°-	
		116 r°, 153 r°-155 r°	51
	XX. 95	41 r°-129	52
	fondo antico 323	114 v°-119 r°	93
Vienna	2358	124 r°-149 v°	6
	2520	37 r°-68 r°	42
	4217	1 r°-9 v°, 29 r°-45 r°	37
	4751	5 v°-212 r°	7
	4948	162 r°-168 r°	27
	5292	199 r°-209 v°	77
	5309	67 r°, 117, 126 v°	141
	5447	1 r°, 24 v°-25 r°,	
		115 v°, 131 r°	141
Wolfenbüttel	270 (237 Helmst.)	385 r°-392 r°	
	402 (367 Helmst.)	31 v°-38 r°	105

2794 (81.4 Aug. 2°)	279 v°-281 v°, 290 r°-291 r°	71
3282 (32.17 Aug. 4°)	224 v°-244 r°	104
3772 (77. 2 Aug. 8°)	11-14v	148

Columbia University
New York City.

LYNN THORNDIKE.

Notes on the History of the Experimental Determination of the Thermal Conductivity of Gases

ABSTRACT

Part I covers the developments in this field from the earliest experiments of PRIESTLEY, circa 1780, until the establishment of the thermal conducting power of gases by MAGNUS in 1860.

Part II covers the important developments from the time of MAGNUS until the present time.

No attempt is made to deal with technical detail or theoretical advance.

PART I

UNTIL THE ESTABLISHMENT OF THE THERMAL CONDUCTING POWER OF GASES

1. A previous paper (1) has dealt with The History of the Concept of Thermal Conductivity. This paper will deal with certain aspects of the history of the experimental determination of the thermal conductivity of gases. Certain points of interest growing out of this study will be mentioned.

2. The first systematic study of the thermal conducting power of gases was undertaken by J. PRIESTLEY about 1780, although he had initially investigated the subject experimentally some ten years earlier. He says (2) : " One of the first things that I proposed to try, with respect to the different kinds of air, was to observe *their power of conducting heat*, and I had a contrivance for that purpose when I was at Leeds. The thing, however, appearing

(1) *Isis*, 20, 246-59, 1933.

(2) " Experiments and Observations relating to various branches of Natural Philosophy," 1st Ed. (1781), vol. 2, pp. 375-378.

to me to be of less consequence than other things that I had in view I deferred it till very lately; when, in consequence of the doctrine of heat becoming, by means of Dr. CRAWFORD's book, the subject of general conversation, I determined to execute what I had so long projected."

3. "For this purpose I prepared the vessel represented in fig. 2 and described in the *Introduction*. The thermometer was a very sensible one, and the scale large, so that I could mark upon it twenty divisions, each larger than half an inch between the temperature of the atmosphere, and a heat much below that of boiling water. After several trials I at length adjusted it in such a manner, that, having filled the vessel with any kind of air, I could plunge it to a certain depth, first in *hot*, then in *cold* water, so that the mercury would rise to the division 20, and fall to that of 6 or 7 in a reasonable time. I had a clock that beat seconds close by me, and was so situated that I could not well make a mistake of more than two seconds, in noting the time when the mercury came to any particular division. The precautions I used to plunge the vessel the same depth in the water, in all the experiments, and to exclude all other differences, except what might be occasioned by the different kinds of air, it would be tedious to recount; and no person conversant in experiments, and who is disposed to repeat them, will need to be so minutely instructed."

4. "I will just observe, however, that in the vessel I used for *hot* water, I always made it boil, and it was so full, that the plunging of my air vessel into it made it run over; and the vessel for cold water was always fresh brought from the same pump. The mouth of the air vessel was in a cup of mercury, always filled to the same height; and by this means I could try in the same manner even those kinds of air, that could not be confined by water."

5. "The best account that I could give of the results of those experiments would be to exhibit them in the form of *tables*, of the time at which the mercury reached all the degrees of the scale, both in ascending and descending; which tables I have drawn up. But this I shall defer until I have an opportunity of repeating all the observations. At present I would only observe, that all the differences were not so striking as I expected to have found them, but that *inflammable* air conducted heat much better

than any other kind of air, the mercury ascending the same space in about half the time that it took up in common air. *Fixed air*, and all the kinds of *acid air* conducted heat considerably worse than common air. *Alcaline air* conducted heat rather better than the acid airs, and dephlogisticated air a little worse than common air, but so little, that I would not answer for the same result in repeating the experiment."

6. It is plain from the foregoing account that PRIESTLEY was not measuring the thermal conducting powers of the various airs. He measured the rate of temperature change of a thermometer surrounded by an enclosed body of gas. The distinction between the rate of cooling and the thermal conducting power had not yet been made. The concepts of specific heat and of temperature were of but recent origin. Convection and conduction had not been identified as separate means of heat transfer. This accounts for the faulty design of the experiment and interpretation of the results.

7. A study of these experiments clearly indicates PRIESTLEY's claim to being a skillful manipulator. His care in planning the operations, his precautions to eliminate errors, his attempts to limit and control the variables, the quantitative recognition of errors not eliminated, the proposed repetition of the work and the restrained and almost charily drawn conclusions all bespeak the great experimentalist.

8. PRIESTLEY originated what, in the hands of later investigators, when suitable concepts had been evolved, became the first, and for years the most popular, method for determining the thermal conductivity of gases. Also, PRIESTLEY was the first to note that hydrogen possessed markedly different thermal properties than did the other common gases.

9. The next systematic investigation of the subject was that of RUMFORD. His experiments, while much more extensive, were similar to those of PRIESTLEY and might easily have been suggested by the earlier ones. In a communication to Sir JOSEPH BANKS, the eminent President of the Royal Society, RUMFORD, then Sir BENJAMIN THOMPSON, describes in detail the thorough investigation which he carried out at Mannheim in 1785 (3). "I have

(3) *Trans. Roy. Soc.*, London, 76, 273-304, 1786.

at length begun the course of experiments upon heat which I have so long had in contemplation; and I have already made a discovery, which, if not new to you, is perfectly so to me, and which I think may lead to a further knowledge respecting the nature of heat."

10. This was the beginning of a program of research which was carried on over a period of more than twenty years. During this time RUMFORD established himself as a manipulator of the first rank and an ingenious designer of apparatus. His method of experimentation differed from that of PRIESTLEY in being less haphazard and aimless. Each series of experiments was specifically designed to decide the point in question and wherever possible his work was quantitative. His manner of reporting his work added to its value. His Essays and other papers seem today prolix, and too much given to minutiae, and his style no longer appeals; but it must be remembered that in their day they were the vogue. From his papers it is readily seen what he did, how he did it, the results obtained and the conclusions drawn. This is good reporting, regardless of the style, and his writings are not only of present interest but also of present value.

11. Recounting his experiments, he says; "It having been my intention from the beginning to examine the conducting powers of the artificial airs or gases, —, I began with fixed air, —." Because his heating and cooling experiments did not check as closely as might be desired, he raises the question "Does air receive heat more readily than it parts with it? This is a question highly deserving of further investigation, and I shall not fail to give it a full examination in the course of my projected inquiries; ..." His investigations were interrupted by an accident which destroyed one of his valuable instruments but at the same time opened up a new question. He details the incident: "Immediately upon plunging the instrument into the boiling water, the mercury began to rise in the thermometer with such uncommon celerity, that it had passed the first division on the tube (which marked the tenth degree, according to REAUMUR's scale) before I was aware of its motion; and having thus missed the opportunity of observing the time elapsed when the mercury arrived at that point, I was preparing to observe its passage of the next, when all of a sudden the stopple closing the end of the cylinder was

blown up the chimney with a great explosion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and destroyed in its fall."

12. "This unfortunate experiment, though it put a stop for the time to the inquiries proposed, opened the way to other experiments not less interesting." RUMFORD suspected that this explosion was due to moisture in the apparatus. This led to the question: "Does humidity augment the conducting power of air?" His conclusions were that "From these experiments it appears, that the conducting power of air is very much increased by humidity."

13. Still extending the field, RUMFORD continues: "Examining the conducting power of air, and of various other fluid and solid bodies, with regard to heat, I was lead to examine the conducting power of the Torricellian vacuum. From the striking analogy between the electric fluid and heat respecting their conductors and non-conductors (having found that bodies, in general, which are conductors of the electric fluid, are likewise good conductors of heat, and, on the contrary, that electric bodies, or such as are bad conductors of the electric fluid, are likewise bad conductors of heat), I was lead to imagine that the Torricellian vacuum, which is known to afford so ready a passage to the electric fluid, would also have afforded a ready passage to heat." After discussing in detail some sixteen experiments, he concludes: "From the results of these experiments it appears evidently, that the Torricellian vacuum, which affords so ready a passage to the electric fluid, so far from being a good conductor of heat, is a much worse one than common air, which of itself is reckoned among the worst: —." This conclusion logically led RUMFORD to consider another phase of the subject: "Finding so great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of density." As usual, he describes his experiments in detail and gives a table of the relative conducting powers of air over a rather wide range of pressures. He concludes: "The result of these experiments, I confess, surprised me not a little; but the discovery of truth being the sole object of my inquiries (having no favorite theory to defend) it brings no disappointment along with it, under whatever unexpected shape

it may appear. I hope that further experiments may lead to the discovery of the cause why there is so little difference in the conducting powers of air of such very different degrees of rarity, while there is so great a difference in the conducting powers of air, and of the Torricellian vacuum. At present, I shall not venture any conjectures upon the subject; but in the meantime I dare to assert, that the experiments I have made may be depended upon." He concludes his first attack upon the problem of thermal conduction by saying that " This is certainly a subject of investigation not less curious in itself than it is interesting to mankind; and I wish that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it."

14. As a result of these experiments, RUMFORD believed that he had shown (*a*), that air conducted heat; (*b*), that different gases had different conducting powers; (*c*), that the Torricellian vacuum did not conduct heat; (*d*), that humidity augmented the conducting power of air; and, (*e*), that the conducting power of air was constant over quite a range of pressure. He maintained the dependability of his experiments but admitted the possibility of different interpretations. He was unwilling to jump at conclusions. " It is certain, that nothing is more dangerous, in philosophical investigations, than to take anything for granted, however unquestionable it may appear, till it has been proved by direct and decisive experiment (4)." With this admonition in mind he continued his studies. He soon distinguished between heat transfer by conduction and that by convection, for the case of fluids. By means of ingenious experiments he demonstrated the convective process and was led to completely revise his opinions, declaring that gases, indeed all fluids, transferred heat only by this new process. He did not use the term " convection " but accurately described and explained the process. He maintains (5) " that although the particles of any fluid, individually, can receive heat from other bodies, or communicate it to them; yet, among these particles themselves, all interchange and

(4) " Essays, Political, Economical, and Philosophical, 1st Am. Ed. (1799), vol. 2, p. 200.

(5) *Ibid.*, p. 202.

communication of heat is absolutely impossible." Further (6), "Heat is incapable of passing through a mass of air, and that it is to this circumstance that its non-conducting power is principally owing."

15. RUMFORD's revised conclusion was almost universally accepted, not merely because of the reputation of the author, but because he had so ably examined the subject experimentally, contriving decisive experiments, the results of which apparently admitted only one interpretation. The eminent JOSEPH BLACK was much struck with the novelty and importance of this opinion and had begun to examine it with his characteristic cautious exactness when his health failed him. The current opinion in the scientific world is well represented by ROBISON's notes in BLACK's Chemistry (7), "It is clearly established by the experiments of Count RUMFORD, that when the internal motion, producible by the inequality of specific gravity, occasioned by heating the lower parts of a fluid, is prevented, the progress of heating is almost insensible, and is no more than we may expect by means of the solid matter of the vessel and by radiation." "There is nothing, therefore, in the opinion offered with great caution by Count RUMFORD, that is incompatible or inconsistent with our most distinct notions on these subjects. It is a fair field of inquiry, most ingeniously laid open to philosophers."

16. Considering the concepts available and the precision of his instruments, RUMFORD's lengthy experimental investigation of thermal conduction stands out as a monumental piece of work, and, for the times, his conclusions were thoroughly justified. He had a good conception of thermal conduction but apparently no conception of conductivity. He first distinguished between conduction and convection and introduced the technique of heating fluids at the upper surface to minimize convection. In the hands of later experimenters this became one of the standard methods used in investigating gaseous conductivity.

17. RUMFORD's conclusions could not long remain unchallenged and the beginning of a lengthy controversy was marked by J. LESLIE's comments in "An Experimental Inquiry into the

(6) *Ibid.*, p. 457.

(7) BLACK's Chemistry; Edited by JOHN ROBISON, 1st Am. Ed. (1807), p. 339.

Nature and Propagation of Heat" (8), published in 1804. He says: "A late ingenious experimenter, who, by the perspicuity and useful tendency of his writings, is deservedly a favorite of the public, has advanced the paradoxical conclusion, that "fluids are non-conductors of heat"; and this strange assertion, from the celebrity of its author, has been treated certainly with more attention and respect than it otherwise merited. If nothing more is meant than, that fluids, as a consequence of their extreme mobility, convey the impressions of heat chiefly by means of their internal motions,—the fact will not be disputed, but, though perhaps more distinctly announced, it can have very little claim to originality. If the proposition, however, be taken in its strict sense, it is most palpably erroneous. Were fluids absolutely incapable of conducting heat, how could they ever become heated? Must we suppose that the particles of a fluid can imbibe heat from those of a solid, and yet not receive it from each other? On mixing cold with hot water, a sort of heterogeneous compound would be formed, each molecule retaining, without participation, its initial and peculiar temperature. And where no such intermixture can take place, how could water, for instance, be heated by the contact of warm air?—But the question really deserves no serious discussion." In spite of LESLIE's cavalier treatment of this conclusion, it was accepted, as far as gases were concerned, for years to come.

18. J. DALTON (9), H. DAVY (10) and P. L. DULONG and A. T. PETIT (11) carried out cooling experiments in which the thermometers were surrounded by gases. While using methods similar to those of PRIESTLEY and RUMFORD, they were confessedly studying the "law of cooling" and not thermal conduction. As T. THOMSON notes in "An Outline of the Sciences of Heat and Electricity" (12), 1830, "It is obvious, that the rate of cooling depends chiefly on the mobility of the gaseous particles. The process is different from the conduction of heat by solid bodies."

(8) 1st Ed. (1804), p. 552.

(9) *Mem. Manchester Lit. and Phil. Soc.*, 5, 373-397, 1802.

(10) *Trans. Roy. Soc.* London, 107, 45-76, 1817.

(11) *Ann. de Chim. et de Phys.*, Part 1 : 7, 113-154, 1818; Part 2 : 7, 225-264, 1818; Especially Part 3 : 7, 337-367, 1818.

(12) 1st Ed. (1830), p. 109.

19. The final step in distinguishing between conduction and the new mode of heat transfer discovered by RUMFORD was in naming the latter. This was done by WILLIAM PROUT in the Eighth Bridgewater Treatise : " Chemistry, Meteorology, and the Functions of Digestion, considered with reference to Natural Theology " (13), published in 1834. He says : " There is at present no single term in our language employed to denote this third mode of the propagation of heat; but we venture to propose for that purpose, the term *convection* (*), which not only expresses the leading fact, but also accords very well with the two other terms. Each of these modes of the propagation of heat possesses certain peculiarities,..."

20. FOURIER, during his work, both experimental and theoretical, on the subject of thermal conductivity, had introduced the concept of conductivity and the terms conductibilité or conducibilité. W. WHEWELL, in his report to the British Association for the Advancement of Science in 1835 objected to these terms, saying (14); " Such terms are obviously improper, except we could apply the adjectives *conductible* or *conducible* to the substances, which it would be a gross solecism to do; but we may say of substances, that they are more or less conduc*tive*, and we may therefore properly speak of their—*conductivity*."

21. T. ANDREWS (15) and W. R. GROVE (16) in 1840, and independently of each other, studied the behavior of a platinum wire electrically " ignited " in an atmosphere of various gases. Both men noticed the exceptional effect of hydrogen on the wire. GROVE carried the investigation farther than ANDREWS; his studies in 1848 being quite thorough and extensive. He says (17) : " In the Philosophical Magazine for December 1845, I pointed out a striking difference between the heat generated in a platinum wire by a voltaic current, according as the wire is immersed in atmospheric air or in hydrogen gas, and in the Bakerian lecture for 1847 I have given some further experiments

(13) 1st Ed. (1834), p. 65.

(*) " *convectio*, a carrying or conveying."

(14) Reports, British Association for the Advancement of Science, 5, 1-34, 1836, particularly, p. 19.

(15) *Proc. Roy. Soc. Irish Acad.*, 1, 465-468, 1841.

(16) *Phil. Mag.*, 27, 442-446, 1845.

(17) 35, 114-126, 1849.

on the subject, in which the wire was ignited in atmospheres of various gases, —." "Although the phenomenon was apparently abnormal, there were many known physical agencies by which it might possibly be explained, such as the different specific heats of the media, their different conducting powers for electricity, or the varying fluency or mobility of their particles which would carry off the heat by molecular currents with different degrees of rapidity."

22. He carried out numerous experiments, eliminating one proposed cause after another. Hydrogen, nitrogen, oxygen, carbon monoxide, carbon dioxide, hydrogen sulphide, oxides of nitrogen, olifiant gas, ether vapor and phosphine were used as gaseous atmospheres. Following a description of his apparatus and experiments, he says: "Upon the whole, we may conclude, from the experiments detailed in this paper, that the cooling effect of different gases, or rather the difference in cooling effect of hydrogen and its compounds from that of other gases, is not due to differences of specific heat; it is not due to differences of specific gravity; it is not due to differences of conducting powers for electricity; it is not due to the character of hydrogen in relation to its transmission of sound, noticed by LESLIE, for reasons which I have before given; it is not due to the same physical characters which occasion one gas to escape from a small aperture with greater facility than another; but it may be, and probably is, affected by the mobile or vibratory character of the particles by which heat is more rapidly abstracted." It is surprising that nowhere in his thoroughgoing interpretation of results is it even suggested that gases might conduct heat. GROVE does, however, suggest an explanation. In an earlier paper (16), he had said: "I have some doubt whether the different gases do not exercise a specific action on the ignited wire, somewhat in the nature of a catalysis; if a wire be brought by the voltaic current to a white heat in atmospheric air, and a vessel of hydrogen inverted over it, the light is as suddenly extinguished as the flame of a candle would be." In his later paper he extends this idea (17). "I incline to think, that, although influenced by the fluency of the gas, the phenomenon is mainly due to a molecular action at the surfaces of the ignited body and of the gas. We know that in the recognized effects of radiant heat, the physical state of the sur-

face of the radiating or absorbing body exercises a most important influence on the relative velocities of radiation or absorption; thus black and white surfaces are, as every one knows, strikingly contra-distinguished in this respect; Why may not the surface of the gaseous medium contiguous to the radiating substance exercise a reciprocal influence? Why may not the surface of hydrogen be as black, and that of nitrogen as white to the ignited wire? This notion seems to me the more worthy of consideration as it may establish a link of continuity between the cooling effects of different gaseous media and the mysterious effects of surfaces in catalytic combinations and decompositions by solids such as platinum."

23. The method of electrically heating a platinum wire, mounted centrally in a narrow tube and surrounded by a gaseous medium was developed by G. MAGNUS, A. SCHLEIERMACHER and others into the third, and today the most popular, method for determining the thermal conductivity of gases.

24. MAGNUS finally established the fact that gases conduct heat. His brilliant investigation was completed in 1860 and reported in POGGENDORF's *Annalen* (18), immediately being translated into English and appearing in full in the *Philosophical Magazine*. He says: "The more immediate inducement to this investigation was a repetition of GROVE's interesting experiment, that a platinum wire is less strongly heated by a galvanic current when surrounded by hydrogen than when it is in atmospheric air or any other gas." Commenting on the previous work of RUMFORD and others, he says: "A conductibility of heat in gases has never been imagined to exist. Although it is in any case very small, it appeared interesting to investigate what influence it might exert, and whether differences existed in the conductibility of different gases; for the deportment of gases is of special importance, not only for the laws propounded by DULONG and PETIT, but also for any theory of the nature of heat."

25. MAGNUS, like RUMFORD and GROVE, describes his apparatus and experiments and considers carefully the alternative interpretation of his results. His conclusion is that "After these results

(18) *Ann. Phys. (Pogg.)*, 112, 497-548, 1861 or *Phil. Mag.*, 22, 1-12, 85-106, 1861.

it cannot be doubted that hydrogen conducts heat, and that in a higher degree than all other gases." While his attention was centered on the anomalous behavior of hydrogen, his experiments established the general proposition of gaseous conduction.

PART II

From the establishment of the thermal conducting power of gases until the present

26. Rumford's conclusion that gases would not conduct heat had been accepted for some seventy years. His similar conclusion regarding liquids had been definitely overthrown by C. Despretz (19) in 1839. Magnus had now clearly shown that gases do conduct heat. However, his views were not universally accepted at once. His former pupil, J. Tyndall, who knew his skill as an experimenter and his caution in drawing conclusions, commented at length upon the subject in his lectures at the Royal Institution and in his "Heat Considered as a Mode of Motion" (20), published in 1863. He says: "The subject of gaseous conduction has been recently taken up by Prof. Magnus, of Berlin, who considers that his experiments prove that hydrogen gas conducts heat like a metal." Commenting on the work of Grove, he continues, "This extraordinary cooling power of hydrogen has been usually ascribed to the mobility of its particles, which enables currents to establish themselves in this gas with greater facility than in any other. Prof. Magnus conceives the chilling of the wire to be an effect of conduction." He discusses the experimental work and the interpretation of the results but concludes: "Beautiful and ingenious as these experiments are, I do not think they conclusively establish the conductivity of hydrogen." In spite of his admonition to his readers that "Theories are indespensible, but they sometimes act like drugs upon the mind. Men grow fond of them as they do of dram-drinking,...", his position in regard to Magnus' work remained unchanged as late as the sixth edition of his book in 1880.

(19) *Ann. de Chim. et de Phys.*, 71, 206-222, 1839.
(20) 1st Ed. (1863), pp. 238-240.

27. Following the establishment of the thermal conducting power of gases, a large number of workers became interested in this field. In the year in which MAGNUS proved the existence of thermal conduction in gases, J. C. MAXWELL published his paper "Illustrations of the Dynamical Theory of Gases" (21). In this paper he calculated, for the first time, a theoretical value for the conductivity of a gas. His development included implications as to the dependence of this property upon temperature, pressure, etc. He includes the statement that "It would be almost impossible to establish the value of the conductivity of a gas by direct experiment, as the heat radiated from the sides of the vessel would be far greater than the heat conducted through the air, even if currents could be entirely prevented."

28. R. CLAUSIUS, in 1862, published an article "On the Conduction of Heat by Gases" (22) in which he speaks of MAXWELL's "interesting memoir" as being "remarkable for the elegance of its mathematical developments." He calls attention to several errors in MAXWELL's treatment and presents a more extensive one of his own. After obtaining his final equation he remarks: "Accordingly, the last equation enables us at once to draw two general conclusions. First. *For a given value of* dt/dx, *the conduction of heat increases with the temperature which the gas has at the place under consideration.*" "Secondly. *The conduction of heat is not affected by the pressure to which the gas is exposed.*—This latter conclusion might lead to obscurity if it were assumed to be applicable to the gas under every conceivable condition of compression or expansion. It must, however, be borne in mind that there are obvious limits to applications of it to conditions of the gas which depart very much from the mean conditions; on the one hand, the gas must not be so much compressed as to produce too great a departure from the laws of permanent gases which have been taken as the foundation for the whole course of reasoning; and on the other hand, it must not be so much expanded that the mean length of excursion of the molecules becomes so great that its higher powers cannot be disregarded." He sums up his views as follows:

(21) Part 1, *Phil. Mag.*, 19, 19-32, 1860; especially Parts 2 and 3, 20, 21-37, 1860.

(22) *Ann. Phys. (Pogg.)*, 115, 1-56, 1862 or *Phil. Mag.*, 23, 417-435, 512-534, 1862; especially pp. 529-534.

"1. Gases conduct heat considerably worse than metals..."
"2. The conducting power for heat depends on the temperature of the gas, and increases in the same ratio as the velocity of sound."
"3. The conducting power for heat is, within certain limits, independent of the pressure to which the gas is subjected"
"4. Light gases conduct heat better than heavy gases."

29. MAXWELL again attacked the problem in a paper presented to the Royal Society in 1866 (23). He calls attention to the memoir of CLAUSIUS and then proceeds to a new approach to the subject. He changes his molecular model from "elastic spheres of finite radius" to "small bodies or groups of smaller molecules repelling one another with a force whose direction always passes very nearly through the centers of gravity of the molecules, and whose magnitude is represented very nearly by some function of the distance of the centers of gravity." MAXWELL determined, by viscosity experiments, that the variation was inversely as the fifth power of the distance. On this new basis he calculates again the conductivity of air, discusses its variation, predicts that oxygen, nitrogen, and carbon monoxide will have approximately the same conductivity, that carbon dioxide will have an appreciably lower conductivity, while hydrogen will have a conductivity seven times as great as that of air.

30. These theoretical investigations and predictions gave a great impetus to the experimental study of the question. In 1871 F. NARR (24) made the first relative determinations using a modified "cooling thermometer" apparatus. His results checked MAXWELL's prediction, finding air and nitrogen to have about the same conductivities, that of carbon dioxide to be definitely lower, and that of hydrogen to be nearly six times that of air.

31. J. STEFAN (25), in 1873, made the first determination of the absolute conductivity. He initially followed a method first suggested by RUMFORD, heating a cylindrical column of air at the top and cooling it at the bottom. His results were unsatisfactory and he forsook this method for the "cooling thermometer"

(23) Scientific Papers, vol. 2, pp. 26-78.
(24) *Ann. Phys. (Pogg.)*, 142, 123-158, 1871.
(25) *Journ. de Phys.*, 2, 148, 1873.

method. In the case of air he found a value in remarkable agreement with that calculated by MAXWELL, also, that the conductivity was constant over a rather wide range of pressure, and that hydrogen had a conductivity seven times that of air.

32. A. KUNDT and E. WARBURG (26) in 1875 investigated the effect of pressure on conductivity, improving the results of STEFAN. They state: "With higher pressures the action of pure heat conduction is defaced by the action of the currents formed, in consequence of gravity, in the unequally heated gas. But when the pressure is lessened the velocity is increased with which, under given conditions, thermometric equilibrium is restored; and therewith the influence of currents recedes." "The values obtained between 10 millims. and 1 millim. mercury-pressure illustrate, on the one hand, the independence of the coefficient of heat-conduction of the pressure within those limits, and, on the other, can be utilized for calculating that coefficient." The attempted elimination of convection effects by reducing the pressure, as done by STEFAN and KUNDT and WARBURG, was based on assumption. Similarly, the conclusion that constancy of experimental values meant constancy of conductivity was based on the assumption that within the pressure range used, convection had been eliminated and MAXWELL's rule held.

33. These same investigators first postulated a discontinuity in the temperature gradient close to the surface of a solid dissipating heat by conduction into a gas. They say: "As in friction the velocities, so here the temperatures of a partition and the contiguous layer of gas differ by a finite value. A diminution, therefore, of the heat flow must first appear, as soon as this difference of temperature commences to exert a perceptible action. On further rarefaction a point is reached from which, onward, the idea of a coefficient of heat conduction loses its meaning, and the flow of heat rapidly decreases as the density diminishes." This same phenomenon was later investigated by M. SMOLUCHOWSKI (27), M. KNUDSEN (28), F. SODDY and A. J. BERRY (29) and others.

---

(26) *Phil. Mag.*, 50, 53-62, 1875.
(27) *Ann. Phys. (Wied.)*, 64, 101-130, 1898.
(28) *Ann. Phys.*, 34, 593-656, 1911.
(29) *Proc. Roy. Soc.*, London, 83 A, 254-264, 1909; 84 A, 576-585, 1910.

34. A. WINKLEMANN (30), in 1875, first determined the temperature coefficient of thermal conductivity. This had been predicted, in different forms, by both MAXWELL and CLAUSIUS. The values obtained by WINKLEMANN did not allow of a choice between the two predictions.

35. By this time a large number of determinations of thermal conductivity had been made. Every investigator determined the absolute value for air and frequently used that as a standard for the relative determinations on other gases. Various gases had been investigated, including air, hydrogen, carbon dioxide, nitrogen, oxygen, several oxides of nitrogen, carbon monoxide, methane and ethylene. The numerical values, though frequently discordant, were usually given to four significant figures. This held even for the crudely determined temperature coefficients. KELVIN (31), in 1878, remarked that : " CLAUSIUS and MAXWELL have given us thermal conductivities of air and other gases, from their splendid development of the kinetic theory, which are undoubtedly trustworthy as somewhat close approximations to the true values, and which it is quite possible are more accurate than we can hope to see obtained from direct measurements of the conduction of heat through gases."

36. A. OVERBECK (32), in 1879, gave some theoretical basis to the practice of eliminating convection by reducing the pressure. In his consideration of the loss of heat by convection he gives the general differential equations for the problem but finds it impossible to solve them for actual cases. He does state that the heat lost by convection, or in conductivity determinations the convection loss, is proportional to the fourth power of the density of the gas.

37. C. CHRISTIANSEN (33), in 1881 and later, introduced the " parallel plate " method for determining conductivities. The essential idea of this method had been suggested by RUMFORD and used by a number of investigators in studying liquids. The method was not generally used and was only revived by

(30) *Ann. Phys. (Pogg.)*, 159, 177-197, 1876.
(31) Mathematical and Physical Papers, Vol. 3, p. 196.
(32) *Ann. Phys. (Wied.)*, 7, 271-292, 1879.
(33) *Ann. Phys. (Wied.)*, 14, 23-33, 1881.

G. W. TODD (34), at the suggestion of POYNTING, in 1909, and again by E. O. HERCUS and T. H. LABY (35) in 1918.

38. A. SCHLEIERMACHER (36), in 1888, introduced the third, the "hot wire," method. This method has been improved in the hands of many able investigators and now is the one in most general use.

39. H. SENFTLEBEN (37), in 1930, investigated the effect of a magnetic field applied perpendicularly to the direction of heat flow. An appreciable effect was found only in the case of oxygen and nitric oxide. W. B. MANN and B. G. DICKINS (38), in 1931, investigated the bearing of molecular structure on the thermal conductivity of various gaseous saturated hydrocarbons.

40. No attempt has been made to mention the work of a large number of men who have made valuable contributions to either the experimental or theoretical side of the problem. A critical review and extensive bibliography of the subject has been prepared by M. TRAUTZ and A. ZÜNDEL (39). Interest in the subject was aroused as early as PRIESTLEY but lapsed with RUMFORD. When reawakened, largely through the efforts of MAXWELL and CLAUSIUS, it proved a popular problem. Great improvement has been made in both the theory of gaseous conduction and in the technique of measurement. Throughout its history the contributions have been almost entirely from the investigators in England and Germany.

41. It may be fairly remarked that in this field theory has outrun experiment. The present theories may be unsatisfactory but the experimental data available for testing them are very discordant and prevent the further development of the theory. Growing out of this historical study of the experimental work are a number of interesting questions. Among them are : (*a*) Is there such a property as thermal conductivity? (*b*) Is the conductivity independent of time? (*c*) Is the conductivity independent of temperature gradient? These questions are historical

(34) *Proc. Roy. Soc.*, London, 83 A, 19-39, 1909.
(35) *Proc. Roy. Soc.*, London, 95 A, 190-210, 1918.
(36) *Ann. Phys. (Wied.)*, 34, 623-649, 1888.
(37) *Phys. Zeit.*, 31, 961-963, 1930.
(38) *Proc. Roy. Soc.*, London, 134, 77-96, 1931.
(39) *Zeit. für techn. Phys.*, 12, 273-284, 1931.

only in a sense and their discussion will have to await a more suitable time and place.

(Massachusetts Institute of Technology, Cambridge, Mass.) ALEX. C. BURR.

INDEX OF NAMES

(The numbers refer to paragraphs.)

ANDREWS, T., 21.
BANKS, J., 9.
BERRY, A. J., 33.
BLACK, J., 15.
CHRISTIANSEN, C., 37.
CLAUSIUS, R. J. E., 28, 29, 34, 35, 40.
CRAWFORD, A., 2.
DALTON, J., 18.
DAVY, H., 18.
DESPRETZ, C., 26.
DICKINS, B. G., 39.
DULONG, P. L., 18, 24.
FOURIER, J. B. J., 20.
GROVE, W. R., 21, 22, 24, 25, 26.
HERCUS, E. O., 37.
KELVIN, Lord, 35.
KNUDSEN, M., 33.
KUNDT, A., 32.
LABY, T. H., 37.
LESLIE, J., 17, 22.
MAGNUS, G., 23, 24, 25, 26, 27.
MANN, W. B., 39.
MAXWELL, J. C., 27, 28, 29, 30, 31, 32, 34, 35, 40.
NARR, F., 30.
OVERBECK, A., 36.
PETIT, A. T., 18, 24.
POYNTING, J. H., 37.
PRIESTLEY, J., 2, 6, 7, 8, 9, 10, 18, 40.
PROUT, W., 19.
ROBISON, J., 15.
RUMFORD, Count, 9, 10, 12, 13, 14, 15, 16, 17, 18, 19, 25, 26, 31, 37, 40.
SCHLEIERMACHER, A., 23, 38.
SENTFLEBEN, H., 39.
SMOLUCHOWSKI, M., 33.
SODDY, F., 33.
STEFAN, J., 31, 32.
THOMSON, T., 18.
TODD, G. W., 37.
TRAUTZ, M., 40.
TYNDALL, J., 26.
WARBURG, E., 32.
WHEWELL, W., 20.
WINKLEMANN, A., 34.
ZÜNDEL, A., 40.

Stoff und Form als Problem der biochemischen Forschung. Eine geschichtliche Betrachtung [1]

Man kann das Wörtchen „und" zwischen die verschiedensten Dinge setzen; Wörter sind geduldig. Aber weil diese Geduld schon oft missbraucht worden ist, so ist der Naturforscher im Prinzip misstrauisch gegen Problemsetzungen, zu denen die Sprache zu verführen vermag. Andererseits hat der Fortschritt in der Erkenntnis der Naturdinge Verbindungen aufgezeigt, wo bis dahin niemand welche vermutet hätte. Denkt man z. B. daran, wie es vor zwanzig Jahren aufgenommen worden wäre, wenn jemand das Thema gestellt hätte : „Die Elektronenbahnen und die Gültigkeit des Kausalitätsgesetzes", und vergleicht damit die Fülle von Material, die heute zu diesem Thema vorliegt, so erkennt man wohl die grosse Wandlung.

So braucht man nicht von vornherein gegen ein Thema „Stoff und Form" misstrauisch zu sein, obwohl es dem einen oder anderen von ARISTOTELES reichlich erörtert und von der Scholastik für allzuviele Worte missbraucht zu sein schiene. Auch will die folgende Betrachtung nicht die Gestalten und Gedanken aus jenen frühen Zeiten noch einmal darstellen; sie will vielmehr Bemühungen aus jüngerer Zeit zu verstehen suchen, denen es darum ging, die Beziehung zwischen den Gestalten und Formen in der organischen Welt zu ihrer stofflichen Eigenart ausfindig zu machen. Dabei wird sich vielleicht ergeben, dass zwar die denkerischen Bemühungen um dieses Problem sehr alt sind, aber dass die experimentelle Beschäftigung mit den hier vorliegenden Aufgaben erst für die Gegenwart und die Zukunft in befriedigendem Ausmasse erwartet werden kann.

(1) Nach Vorträgen in Leverkusen, 13. V. 1932, und Heidelberg, 11. XI. 1932.

Es gibt von jeher zwei einander gegensätzliche Betrachtungsweisen für diese Dinge, je nachdem, ob man die Erklärung von oben oder von unten her versucht, vom höchstorganisierten Lebewesen oder vom einfachsten anorganischen Gebilde aus. Dann sieht der Eine, wie Seele und Wille sich den Körper bauen, sei es nun im Menschen oder sei es im Kristall. Der Andere vermeint im Lebewesen auch und nur dieselben mechanischen Kräfte wirken zu sehen wie im Gas, im Stein, im Wasser. In der Natur herrscht Einheit—von diesem Gefühl gehen solche Forscher aus und lassen sich von ihm zur Ganzheitsschau hinreissen, mehr ins Reich der Dichtung als der reinen Forschung. Müssen es „Kristallseelen" sein, die den Stoff in die Form des Kristalls gestalten? Darf man darauf erwidern: auch die „Gitterkräfte" seien geheimnisvoll, und man benutze nur ein beziehungsärmeres Wort für dieselbe Sache, als wenn man von Kristallseelen spräche? Eine solche Erwiderung würde nur den kennzeichnen, der die dichterische Zutat zum einen wie zum andern Begriffe nicht unterlassen könnte. Gitterkräfte, mit denen die Strukturen der Kristalle erklärt werden, sollen nicht nur die chemische Art des Natriums und des Chlors je für sich bezeichnen, sondern auch zusammenfassend die räumlich ordnenden Beziehungen zwischen ihren Atomen darstellen, wenn diese im kristallisierten Kochsalz wirken. Diese Kräfte, die noch weiterhin mit Atomradien und Valenzwirkungen zu tun haben, sind nicht fremde Kräfte *über* dem Stoff, sondern die Kräfte *aus* dem Stoff. Durch sie gesellt sich immer mehr von dem gleichen Stoff zueinander; so wachsen grosse gleichmässige Kristalle heran, in deren Gitter sich auch fremde ähnliche Stoffe unter besondere Bedingungen fügen. Seit durch BERZELIUS die chemische Mineralogie geschaffen wurde, hat man die einfache Grundauffassung: der Stoff bestimmt die Form, da die Form ein Kennzeichen des Stoffes ist, sogleich auch mit den Abänderungen versehen müssen: dass ein Stoff verschiedene Formen und verschiedene Stoffe eine gemeinsam gleiche Form haben können. Auch dies geschieht nach bestimmter Regel mit definierten Umwandlungsintervallen nach einfachen Gesetzen.

Ganz anders baut sich der Organismus auf. Hier wechseln Stoff und Form in geringen Abständen; auf Häute folgen Röhren, in ihnen bewegen sich Flüssigkeiten, Knorpel und Fäden leiten

über zu festen Konkretionen. An der einen Stelle sind Kohlenhydrate in vorwiegender Menge anwesend, an einer anderen mischen sich in reichlichen Anteilen Eiweisstoffe hinzu, Fette lagern darin und daneben und umhüllen mineralreiche Gebilde. Ist es der Gehalt an einem bestimmten Eiweisstoff, der die ganze Stoffschar zur Milz formt? Warum tritt das Gemisch von Kalziumphosphat und Kalziumkarbonat, das mit Gelatine vereinigt ist, das eine Mal in Form der langen Röhrenknochen, das andere Mal in Knorpeln auf? Kann man durch Zufügen bestimmter Mengen von Kalziumfluorid aus diesem Material die Form der Zähne entstehen lassen, auch ausserhalb des Organismus? Wird an der einen Stelle im Kiefer ein anderer formender Stoff der wachsenden Zahnmasse beigegeben, damit sie ins Breite wächst, zum Unterschied von demjenigen formierenden Stoff, der den Eckzahn erzeugt? Die Beantwortung solcher Fragen hätte nicht nur naturphilosophischen, sondern auch recht hohen praktisch-medizinischen Wert.

Man kann freilich in philosophischem Überschwang über jedes naturwissenschaftlich erreichbare Ziel hinauseilen und behaupten, dass auch mit der Auffindung solcher stofflichen Verschiedenheiten und sogar Bedingtheiten noch lange keine „wirkliche Erklärung" zu gewinnen sei. Das Rätsel bestehe fort, ob man nun weiss, dass sich die Lebensenergie einer Auswahl an Materie bedient, um seine geformten Gebilde zu erzeugen; denn was ist es, was sich dieser stofflichen Eigenschaften und Naturgesetze zu seinen Aufgaben bedient? Ob es den Stoff direkt oder vermöge einer besonders feinen Zusammenstellung der Bestandteile formt—es formt, es ist tätig, es ist unabhängig, ausserhalb und oberhalb des Stoffes.

Man kann vielleicht vier besondere Arten von Erklärungsweisen auf diesem Gebiete unterscheiden:

1. Lebenskraft unbeschränkt frei formend;
2. Lebenskraft Stoffeigenschaften benutzend;
3. Stoffgesetze durch die Lebenskraft gesetzmässig modifiziert;
4. Stoffgesetze allein bestimmend.

Wenn man diese vier Fälle durch Schlagwörter bezeichnen will, so bieten sich die folgenden dafür an:

1. Absoluter Vitalismus;
2. Chemischer Vitalismus;
3. Legaler Vitalismus;

4. Absoluter Chemismus (nicht : Materialismus).

So weitreichende Erklärungsweisen pflegen in der Form des Entweder-Oder formal sehr scharf, aber sachlich sehr unzureichend einander gegenüber zu stehen; denn hier können nicht logische Gründe ohne die Untersuchung der Tatsachen entscheiden. Die Tatsachen aber sind auf diesem Gebiete ausserordentlich schwer festzustellen. So wird man sich nicht wundern, aus der Vergangenheit recht verschiedenerlei Ansichten darüber übermittelt zu sehen, wie die Eigengesetzlichkeit des Stoffes und die lebendige Gestaltung im Organismus sich zueinander verhalten.

Bei den *Chemikern* der Vergangenheit scheint die Ansicht von einer beherrschenden Rolle der Lebenskraft häufiger vertreten zu sein, als diejenige von einer Selbstgestaltung des Stoffes. Das ist deshalb merkwürdig, weil auch die alten Elementenlehren zu Zeiten eines PARACELSUS oder ROBERT BOYLE durchaus nicht überrascht sind, dieselben vier oder fünf Elemente wie im Anorganischen auch in Pflanzen- und Tierkörpern wiederzufinden.

Die Methoden, nach denen diese Elemente aufgefunden wurden, waren Extraktion und Destillation. Beide dienten auch dazu, um die Träger der besonderen Heilkräfte aus Pflanzen- und Tierteilen abzusondern. Die heilenden Kräfte haben, wie man durch Erfahrung wusste, in bestimmten Organen der betreffenden Tiere oder Pflanzen ihren Sitz. Schon durch ihre äussere Form offenbaren solche Organe ihre Kräfte : dies ist der Inhalt der Signaturenlehre, der z.B. PARACELSUS anhängt, die auch RUDOLF GLAUBER hochhält : „Die Signaturen der Kräuter nun betreffend, durch welche uns Gott deren verborgene Kräfte zu erkennen gibt..." (2). Die äussere Form, die danach die verborgenen Kräfte anzeigt, hat aber keinen dinglichen Zusammenhang mit ihnen. Nicht die Kraft, die dem Semen fraxini seine Wirkung auf den Calculus gibt, bringt diesen Samen in die kennzeichnende Form. Die Wirkung ist auch nicht an die Form des Organs gebunden, sondern kann daraus unter Zerstörung des Organs ausgezogen werden. ANDREAS LIBAVIUS beschreibt solche Zubereitungen (3). RUDOLF GLAUBER hebt den Vorteil hervor, den sie

(2) RUDOLF GLAUBER, „Ph. spagiricae" 1. Theil, S. 20 f. (Op. omnia, Frankfurt, 1658).

(3) ANDREAS LIBAVIUS, „Alchymistische Practic, das ist von künstlicher Zubereitung der vornembsten chymischen Medizinen", Frankfurt, 1603.

gewähren : man braucht nicht das ganze Heilkraut einzunehmen und damit viel unnützes Zeug in den Magen zu bringen, besonders da man es mit viel Bier oder Wein hinunter spülen müsste; in den Extrakten hat man die ganze wirksame Kraft des Krautes ohne Ballast und in jahrelang haltbarer Form (4).

So herrscht hier die echt chemische Vorstellung von der Isolierbarkeit der wirkenden „Kräfte" in der Form einzelner Stoffe. Dennoch findet die Frage, woher diesen Organismenteilen ihre arzneiliche Kraft kommt, in dieser Zeit eine hauptsächlich *religiöse* Antwort. Daneben gibt es eine Art *philosophischer* Antworten, wofür ein kennzeichnendes Beispiel bei LEIBNIZ zu finden ist. Gegenüber Missverständnissen, denen seine Monadenlehre begegnete, führte er eine Unterscheidung zwischen dem reinen Stoff und dem Körper, dem bekleideten Stoff ein. Der reine Stoff ist nur passiv; dem Körper aber wird noch die „erste Entelechie oder das tätige Prinzip" zugesprochen. „Dies tätige Prinzip, diese erste Entelechie ist in Wahrheit das Lebensprinzip, das... auch mit der Fähigkeit des Vorstellens begabt ist... Insofern ich nun überall im Stoff diesem hinzugefügte tätige Prinzipien annehme, nehme ich auch allenthalben durch den Stoff verbreitete Lebensprinzipien oder Prinzipien des Vorstellens, also Monaden, und wenn ich so sagen darf, metaphysische Atome an, die keine Teile haben und auf natürlichem Wege nie entstehen noch vergehen können " (5).

Eine *chemische* Beantwortung verlangte Grundlagen, wie sie im wesentlichen nicht vor dem späten achtzehnten Jahrhundert gewonnen wurden. Die Chemie der Pflanzen und der Tiere begann erst um diese Zeit bestimmte einzelne Ergebnisse zu liefern. FOURCROY gibt in seinem „Système" eine Übersicht über die Entwicklung dieser Gebiete, die allerdings nicht ganz frei ist von jenem Hervorheben der eigenen epochalen Leistung, wie es nach MARCELLIN BERTHELOTS eigenen Einsichten mit dem französischen Forschungsbetrieb oft in besonders ausgeprägtem Masse verbunden sei. Wir werden uns davon nicht abhalten lassen, die Kraft zur zusammenfassenden Kritik in FOURCROYS

(4) „Opera omnia", 1. Theil, S. 4 f.

(5) LEIBNIZ, „Kleinere Philosoph. Schriften", (Ausgabe : Reclam, Leipzig), S. 209 f.

Darstellung zu bewundern. Noch bis zum Jahre 1784 habe sich die Pflanzenchemie darauf beschränkt, „die verschiedenen Produkte, wie sie in der Pflanze vorhanden sind, zu extrahieren und zu reinigen, ihre besonderen Lösungsmittel festzustellen, die Wege zu ihrer genauen Trennung zu suchen, ihre wichtigsten Unterscheidungsmerkmale zu erfassen, und dies mehr um sie unseren Verwendungen dienstbar zu machen, als in der Absicht oder auch nur in der Hoffnung, ihre Eigenart exakt zu erkennen". Die Gedanken, die man dazu äusserte, seien Irrtümer oder blosse Hypothesen gewesen. Grosse Hoffnung setzte man auf die Untersuchung der Pflanzenasche, und viel Arbeit verwendete man darauf, aber vergebens und nutzlos. Erst als die Anwendung des Feuers durch diejenige der schonenden Extraktion mit kaltem Wasser ersetzt wurde, begann man wesentliche Fortschritte in der Chemie der Pflanzenstoffe zu machen. Diese Fortschritte führten um die Jahrhundertwende zu der Erkenntnis, dass die Stoffe der Pflanzen zumeist dreifache Verbindungen aus Kohlenstoff, Wasserstoff und Sauerstoff sind. „Die chemische Kunst vermag zwar die Grundstoffe eines pflanzlichen Gebildes zu isolieren und aus dem Zusammenhang zu trennen; sobald sie auf dieses Gebilde einwirkt, beginnt sie es damit, dass sie sein Gleichgewicht ändert, und so schwach auch ihre Bewirkung sein mag, zielt sie doch darauf, die Zusammensetzung zu denaturieren..." (6). An diesen Äusserungen von FOURCROY wird man im Hinblick auf jüngste erkenntnistheoretische Errungenschaften auch den Hinweis auf die vom Beobachter ausgehenden Einflüsse recht bemerkenswert finden.

Zu ungefähr derselben Zeit im späteren achtzehnten Jahrhundert beginnt auch die Tierchemie neue exakte Grundlagen zu gewinnen. Aus Blut und Urin werden charakteristische Stoffe von ROUELLE isoliert; SCHEELE lehrt organische Säuren kennen; RÉAUMUR, SPALLANZANI zeigen auflösende Wirkungen des Magensaftes. Sehr wichtig wirkt der Hinweis BERTHOLLETS auf die Rolle des Stickstoffs in den Substanzen des tierischen Organismus. Durch CRAWFORD und andere wird die Atmung als Quelle der Wärme des Tierkörpers erkannt. Mit den Methoden der Chemie dringt

(6) A. F. FOURCROY, „Système des Connaissances chimiques", Paris 1801, vol. 7, S. 55 f.

man allmählich zur Erkenntnis einiger der vom Organismus hergestellten Stoffe vor. Da erhebt sich die Frage nach dem Verhältnis zwischen der Nahrung und der Körpersubstanz, die daraus gebildet wird. Die Antworten sind von charakteristischer Allgemeinheit. Ihr Grundton ist ungefähr derjenige, der in FOURCROYS Zusammenfassung zu finden ist : „... dass die pflanzliche Substanz sich in den eigenen Körper des Tieres umwandelt, also tierische Substanz wird durch eine dem Körper dieses Tieres eingeborene Kraft, welche man Kraft der Animalisation nennt " (7).

PARACELSUS hatte die Umwandlung der Nahrung noch der Aufsicht und Wirkung eines besonderen Wesens, des Archeus, unterstellt, dessen Rolle bei dem Phlogistiker STAHL zum Teil von einer Anima geleistet wurde. Demgegenüber bedeutet es wohl einen Fortschritt, wenigstens zu einer „eingeborenen Kraft" zu gelangen, wenn sie auch im übrigen unerklärlich war. Die Folge : Person—Geist—Kraft kennzeichnet auch sonst oft die Entwicklung des wissenschaftlichen Erklärens.

Die Anordnung der pflanzlichen und tierischen Stoffe ist bei FOURCROY, ebenso wie noch etwa ein Vierteljahrhundert später bei BERZELIUS, durchaus auf die anatomisch gefundenen Formen und auf die physiologischen Einheiten begründet. Im übrigen zeigt der Vergleich dieser beiden Versuche einer übersichtlichen und vollständigen Darstellung die grossen Fortschritte in der Erkenntnis der Einzelheiten; es gelingt immer mehr, das zunächst nur botanisch oder zoologisch Verschiedene auch durch gewisse chemische Besonderheiten zu kennzeichnen. Ein wichtiges gedankliches Hilfsmittel bot dabei die Analogie zum Anorganischen. Sie erleichterte die Begriffsbildung für das so ungeheuer viel kompliziertere Gebiet der „organischen" Chemie und lud dazu ein, die in den Lebewesen gefundenen Stoffe immer weiter von ihrem Ursprungsorte abzusondern, bis aus der Chemie des Lebendigen eine Chemie der Kohlenstoffverbindungen wurde (8). Aber über der Zugänglichkeit und Abtrennbarkeit der einzelnen Stoffe vergessen die grossen Forscher nicht das Problem ihrer Zusammenhänge. Mögen dafür auch häufig ans Mystische grenzende Vorstellungen mitbenutzt werden, so geht das Bemühen

(7) Ebenda, Vol. 9, S. 5.
(8) Vgl. P. WALDEN, *Ztschr. angew. Chemie*, 40, S. 1 (1927).

doch darum, die Grenzen seiner Erkennbarkeit und Einbeziehung ins Bereich des Experimentellen abzutasten.

BERZELIUS sieht jene grosse, nur im lebendigen Zusammenhange wirksame und unsrer Erkenntnis unzugängliche Kraft stoffliche Umwandlungen zuwege bringen, die im Vergleich mit der Laboratoriumskunst als reine Wunder erscheinen müssen. „Das Wesen des lebenden Körpers ist folglich nicht in seinen unorganischen Elementen begründet, sondern in etwas Anderem, welches die unorganischen, für alle lebenden Körper gemeinschaftlichen Elemente zur Hervorbringung eines gewissen, für jede besondere Art bestimmten und eigenen Resultats disponiert. Dieses Etwas, welches wir Lebenskraft nennen, liegt gänzlich ausserhalb der unorganischen Elemente, und ist nicht eine ihrer ursprünglichen Eigenschaften, wie Schwere, Undurchdringlichkeit, elektrische Polarität u. a. m.; aber was es ist, wie es entsteht und endigt, begreifen wir nicht..." (9). Wir haben dennoch einen gewissen Zugang zu dem geheimnisvollen Reich, in dem die Lebenskraft herrscht; denn auch für die Stoffe der belebten Natur gelten die Gesetze der Verbindung ihrer Elemente nach bestimmten Proportionen. Andere vom Anorganischen her bekannte Gesetze erscheinen im Organischen verändert wieder. So ist der elektrochemische Zustand dort anders als im Anorganischen, verändert durch die „eigentümliche Beschaffenheit des lebendigen Organs, und es wäre, von diesem Gesichtspunkte aus betrachtet, nicht unmöglich, dass in der organischen Natur zwei Körper vorkommen könnten, welche aus gleichen Elementen und zu gleichen relativen Gewichten beständen, welche aber durchaus keine gleichen Eigenschaften hätten" (10).

Hiernach gestaltet also die Lebenskraft den Stoff frei, wenn auch nicht von allen seinen eigenen Gesetzen unabhängig. Der Stoff ist im wesentlichen tot und einflusslos, etwa wie jener „erste Stoff" von LEIBNIZ, der nur Leiden und kein Tun kennt. Die Lebenskraft bildet die Zustände aus, in die der Stoff gerät, sie formt ihn also auch, wie sie es braucht. Das ist die Freiheit und Macht der Lebenskraft; bei modernen Naturphilosophen der vitalistischen Richtung findet man sie ähnlich wieder.

(9) J. J. BERZELIUS, „Lehrbuch" übersetzt v. F. WÖHLER, Bd. 3, 1. Abt., S. 136 f. (Dresden 1827).

(10) Ebenda, S. 143.

Die besondere Formung durch die Lebenskraft kennzeichnet sich darin, dass es ganz andere Grundfiguren sind, die den lebendigen Stoff auszeichnen. Gegen die Versuche, Analogien mit dem Anorganischen zu weit zu führen, verweist BERZELIUS eindringlich auf den Unterschied zwischen den im Anorganischen vorherrschenden geometrisch regelmässigen Figuren und all den Fasern, Häuten, Röhren, welche die Formen des Stoffs der Lebewesen sind. Allerdings haben DUMAS, PRÉVOST, MILNE EDWARDS bei ihren mikroskopischen Untersuchungen in den grossen Formen kleine Strukturen gefunden, kugelige Teilchen, die den Autoren als die „organischen Moleküle" erschienen (11). Hier kündigt sich ein wichtiges Thema für spätere Untersuchungen an; zu BERZELIUS' Zeiten hatte man noch nicht viel davon.

So werden auch weiterhin zunächst Formen der Organismen genauer beschrieben und einzelne ihrer Stoffe besser erkannt; eine Beziehung zwischen Stoff und Form schafft aber nur die Lebenskraft, mit der man allzuviel und darum nur allzu allgemein erklären kann. LIEBIG benutzt diese Art des Erklärens in viel ausgedehnterem Masse als BERZELIUS; denn LIEBIGS rationalistischere Betrachtungsweise möchte das Bereich des vernünftig Erklärbaren viel grösser erscheinen lassen, als es das Bereich des wissenschaftlich Erkannten damals schon war. Um die organische Chemie auf Physiologie und Pathologie in ausgedehntem Masse anwenden zu können (12), braucht er deshalb an vielen wichtigen Stellen die Lebenskraft. Bei der Entwicklung von Samen und Ei äussert sich „die in Bewegung übergehende Kraft... in einer Reihe von Formbildungen... Die Kraft heisst Lebenskraft". Ihr Arbeitsgebiet ist auf das rein Stoffliche beschränkt; denn für höhere geistige Tätigkeit sorgt eine „Kraft, welche durchaus verschieden ist und nichts gemein hat mit der Lebenskraft" (13). Dafür, dass sie ein so beschränktes Bereich beherrscht, weiss sie sich darin sehr gut chemisch zu helfen. „In der Fettbildung schafft die Lebenskraft sich selbst ein Mittel, um dem Mangel an Sauerstoff und an der zu den vitalen Prozessen nötigen Wärme zu begegnen". Der Tierorganismus kann zwar seine

(11) Ebenda, Bd. 4, 1. Abt., S. 5 ff.

(12) LIEBIG, „Die organische Chemie in ihrer Anwendung auf Physiologie u. Pathologie", 1842.

(13) Ebenda, S. 1 und 5.

Körpersubstanz aus den Bestandteilen seines Blutes aufbauen; aber „sein Blut muss ihm, bis auf die Form, fertig gebildet dargeboten werden, und wenn dies nicht geschieht, so ist damit der Blutbildung und dem Leben eine Grenze gesetzt" (14). Allerdings können aus den Bestandteilen des Blutes neue Verbindungen entstehen, beispielsweise dadurch, dass Sauerstoff und Wasser hinzutreten. „Die nämliche Lebenskraft, die wir in der Pflanze als eine beinah unbegrenzte Fähigkeit der Zunahme an Masse kennen, verwandelt sich in dem Tierkörper in bewegende Kraft (in einen Strom von Lebenskraft), und eine wunderbare und weise Ökonomie bestimmt zur Ernährung des Tieres nur solche Stoffe, die eine mit den Organen der Krafterzeugung (dem Muskularsystem) identische Zusammensetzung besitzen" (15).

Wenn die Lebenskraft danach die besonderen Formen des Organischen zu schaffen vermag, so lässt sich vielleicht auch eine Umkehrung des Abhängigkeitsverhältnisses zwischen Stoff und Form behaupten. „Nichts hindert uns, die Lebenskraft als eine besondere Eigenschaft zu betrachten, die gewissen Materien zukommt, und wahrnehmbar wird, wenn ihre Elementarteilchen zu einer gewissen Form zusammengetreten sind" (16). So erschiene die Lebenskraft als eine Integraleigenschaft—ein wichtiger Gedanke, der von LIEBIG aber nicht weiter verfolgt wird. Vielmehr kommt er trotz des Versuchs weitgehend rationalistischer Darstellung zu einem irrationalen Endergebnis : „In welcher Form, auf welche Weise die Lebenskraft die mechanischen Effekte im Tierkörper bewirkt, ist gänzlich unbekannt und wird durch Versuche so wenig ermittelt werden können, wie der Zusammenhang der chemischen Aktion mit den Bewegungserscheinungen, die wir mit der galvanischen Säule hervorzubringen vermögen" (17). Fast ein halbes Jahrhundert vor EMIL DUBOIS REYMOND wird hier implicite ein *Ignorabimus* als Antwort auf die Endfragen der Wissenschaft ausgesprochen. Da es sich bei einem wissenschaftlichen Geist wie LIEBIG und seinem späteren Nachfolger auf solchen Gedankengängen in Wirklichkeit nur darum handelt, für den nicht wissenschaftlichen Gehalt der Fragestellung die

(14) Ebenda, S. 96 u. 98.
(15) Ebenda, S. 235.
(16) Ebenda, S. 213.
(17) Ebenda, S. 236.

wissenschaftliche Unbearbeitbarkeit auszusagen, so ist in diesen Behauptungen im wesentlichen nur eine Tautologie enthalten. Man wird auch sonst in LIEBIGS Darlegungen kaum einen wesentlichen Fortschritt in der Lösung des uns hier gestellten Problems über BERZELIUS hinaus erkennen können.

Einige bemerkenswerte neue Ansätze enthält das kurz nach LIEBIGS eben erwähnter „Physiologie" erschienene Buch seines zeitweiligen Gegners G. I. MULDER aus Utrecht (18). Der Satz : gleiche Ursache, gleiche Wirkung führt MULDER zu dem Schluss, dass eine und dieselbe Substanz auch nur in einer einzigen Form auftreten könne (19). Das ist wohl ein zu weitgehender Schluss aus einer zu allgemeinen Prämisse. Ebenso behauptet MULDER zu viel für den damaligen Stand der Wissenschaft, wenn er sagt : „Man hat beobachtet, dass geringe materielle Unterschiede die Bildung verschiedener Formen veranlassen, und dass anderseits kleine Differenzen in der Form wesentliche materielle Verschiedenheiten zur Folge haben" (20). Hier wird vielmehr eine allgemeine Beziehung vielleicht aus logischen Gründen vorweggenommen und ein in Wirklichkeit zu kleiner Bestand an experimentellen Ergebnissen als allgemeiner Beweis angesehen. So meint er, dass kleine Verschiedenheiten im Gehalt an basisch-phosphorsaurem Kalk die Haut in Knochen verwandeln; der Unterschied zwischen Eiweiss und Faserstoff des Blutes sei durch „den Mehrgehalt des einen an drei Zehntel Prozent Schwefel" bedingt ! Einen Unterschied von dieser Grösse hätte MULDER als innerhalb der damaligen Fehlergrenzen liegend erkennen müssen. Seine Behauptungen sind also höchstens Hypothesen; aber sie konnten doch eine starke Anregung geben, nach diesen Richtungen hin weiter zu forschen, ähnlich wie die Hypothese von PROUT über die „Urmaterie" aller Elemente dem Streben nach grösster Genauigkeit bei der Bestimmung der Atomgewichte eine besondere Bedeutung gab, oder wie die Feststellung von in kleinsten Mengen vorhandenen Mineralbestandteilen in neuester Zeit erhöhte Wichtigkeit und gesteigerte Bearbeitung gewann, als die besonderen physiologischen Wirkungen von kleinsten

(18) „Versuch einer allgemeinen physiologischen Chemie", Braunschwig 1844-51.
(19) Ebenda, S. 363.
(20) Ebenda, S. 364 f.

Mengen Bor, Jod, Mangan und anderen Elementen erkannt wurden.

In manchen Organen findet man besondere Stoffe abgelagert, z.B. Kalziumoxalat oder ätherische Öle. PAYEN hatte diese Bildungen der Tätigkeit besonderer Organe dafür zugeschrieben; aber das ist nach MULDER überflüssig. Auch in der Pflanze kann Kalziumoxalat auf rein chemischem Wege ohne Zuhilfenahme einer besonders gearteten pflanzlichen Arbeit gebildet werden, und die Verschiedenheiten unter den ätherischen Ölen können besser als aus Unterschieden zwischen den sie abscheidenden Organen erklärt werden durch „kleinere Modifikationen der Umstände..., unter die ein und derselbe Zellinhalt treten kann" (21).

In dieser Weise versucht MULDER, normales chemisches Geschehen auch im Organismus festzustellen. Das geht so weit, dass für Gebilde wie Haut oder Haar chemische Formeln aufgestellt werden : Die Zusammensetzung der Oberhaut wird durch die Formel 5 ($C_{40}H_{66}N_{12}O_{15}$) S wiedergegeben; aber es ist, „wenigstens für jetzt, unmöglich, den Beweis für ihre Richtigkeit zu liefern" (22). Eine chemische Formel, die man nicht beweisen kann, oder eine Lebenskraft, von der man nicht weiss wie sie wirkt—zwischen diesen beiden Extremen hatte damals der Biochemiker zu wählen. In den neunzig Jahren, die seitdem vergangen sind, ist wenigstens für den ersten Teil eine wesentliche Verbesserung eingetreten (23).

MULDER betrachtet ein gestaltetes Organ als einen chemisch definierbar einheitlichen Stoff. Die organisch gewordene Gestalt bildet eine neue chemische Einheit, nicht nur ein Beisammensein von Stoffen, das erst nach der Zerlegung zum Gegenstand der Chemie würde : so etwa ist MULDERS Ansicht zu kennzeichnen. Von da nehmen auch die ausgedehnten Untersuchungen ihren Ausgang, die er gemeinsam mit DONDERS unternahm, um verschieden gestaltete Gewebe zu isolieren und chemisch zu definieren oder, wie auch beim Speichel und anderen Sekreten, „die mikro-

(21) Ebenda, S. 411 ff.

(22) Ebenda, S. 551.

(23) SCHOPENHAUER allerdings wusste auch schon genau über die Lebenskraft Bescheid : „An sich ist jene Lebenskraft der Wille". Vgl. „Parerga u. Paralipomena", 2. Bd., 1851; i. d. Ausgabe v. Reclam, Leipzig, Bd. V, S. 177.

skopische Untersuchung mit der chemischen zu vereinigen" (24). Die Form der Pflanzenzellen wird oft nicht durch ihre Inhaltsstoffe bestimmt; umgekehrt hat die Zellform Einfluss auf den Zellinhalt, indem sie die Aufenthaltsdauer des Zellsaftes und dadurch die chemischen Umsetzungen darin mit beeinflusst. Aber dass die grossen festen Formen im Pflanzenreiche „von der Materie, den Eigenschaften und Kräften, mit welchen die Materie vom Schöpfer ursprünglich begabt wurde, abhängig sind, versteht sich von selbst, allein es ist uns bis jetzt noch nicht verliehen, einzusehen, auf welche Weise" (25).

Zunächst kommt es für diese Auffassungen noch weitgehend darauf an, welche Überzeugungen man von vornherein mitbringt, und sie erst bestimmen die Richtung der experimentellen Arbeit; aus deren Ergebnissen allein kann man aber noch nichts entscheiden. Solche Überzeugungen sind es auch, die z.B. Ernst Haeckel bei seinen Äusserungen zu diesem Problem leiten. „Alle Lebenserscheinungen der Gestaltungsprozesse der Organismen sind ebenso unmittelbar durch die chemische Zusammensetzung und die physikalischen Kräfte der organischen Materie bedingt, wie die Lebenserscheinungen der unorganischen Kristalle, d.h. die Vorgänge ihres Wachstums und ihrer spezifischen Formbildung, die unmittelbare Folgen ihrer chemischen Zusammensetzung und ihres physikalischen Zustands sind" (26). Wir wissen, „dass diese höchst einfachen Anfänge aller organischen Individuen ungleichartig sind, und dass äusserst geringe Differenzen in ihrer materiellen Zusammensetzung, in der Konstitution ihrer Eiweissverbindungen genügen, um die folgenden Differenzen ihrer embryonalen Entwicklung zu bewirken" (27). Von einem solchen Wissen kann man wohl sagen, dass es auch heute noch nicht erreicht und verwirklicht worden ist (28).

Wenn aber subjektive Überzeugungen auf diesem Gebiete den Inhalt eines zu gewinnenden Wissens vorwegnehmen, dann kann

(24) „Physiologie", S. 943.

(25) Ebenda, S. 785.

(26) „Natürliche Schöpfungsgeschichte", 4. Aufl., S. 295 f.

(27) „Generelle Morphologie", Bd. I, S. 198.

(28) Vgl. Fr. Steinecke, „Naturwiss." 1925, S. 853. Hans Friedenthal hatte 1900 auch nicht eigentlich einen Parallelismus zwischen Eiweissverwandtschaft und Stammesverwandtschaft angenommen, zu dem auch spätere Forschungen nicht führten.

man nicht erwarten, dass sie sich nach dem wirklich erreichten Erkenntnisstande richten. Deshalb gibt es wohl sehr wenige Beispiele dafür, dass Meinungen über das Verhältnis zwischen Lebenskraft und Chemismus durch Forschungsergebnisse verändert wurden. So ist der Fall bei CHARLES GERHARDT besonders bemerkenswert, der um 1853 zwischen jenen beiden Kräften nur absoluten Gegensatz sah, aber später, nachdem ihm neue Synthesen von Stoffen aus dem Pflanzen- und Tierreiche bekannt wurden, „die natürlichen Verbindungen und die künstlichen Produkte unsrer Laboratorien... als die Glieder derselben Kette, welche dieselben Gesetze zusammenhalten..." ansprach (29).

Die Erforschung der chemischen Natur „organischer" Stoffe stand in der zweiten Hälfte des neunzehnten Jahrhunderts so stark im Vordergrunde, brachte so viel Aufgaben und glückliche Lösungen, dass die Fragen nach dem Zusammenhang zwischen Stoff und organischer Form in den Hintergrund traten. Viel wichtiger war jetzt die Feststellung, in welchen Formen die Atome der Elemente sich zu den komplizierten Molekülen verbanden. Man versteht den Stolz, mit dem ein ADOLF VON BAEYER 1883 berichten konnte, nach vielen Bemühungen sei „jetzt der Platz eines jeden Atoms im Molekül dieses Farbstoffs (Indigo) auf experimentellem Wege festgestellt". Wenn es auf weiten, „unnatürlichen" Wegen gelungen war, die gegenseitige Stellung der Atome im Molekül natürlich vorkommender Zucker, Farbstoffe, Terpene festzustellen, dann schloss sich höchstens die Frage an, welche chemischen Wege die Pflanze oder das Tier für die Bildung dieser Stoffe benutzten. Bei den Physiologen und physiologischen Chemikern wirkten die Gedankengänge LIEBIGS oder MULDERS nach. Eine programmatische Äusserung von ALBRECHT KOSSEL aus dem Jahre 1906 knüpft z.B. inhaltlich an MULDER an, wenn es dort heisst: „... es ergibt sich mit Wahrscheinlichkeit, dass jeder Art von Gewebszellen, z.B. den roten Blutkörperchen, den Zellen der Leber, der Speicheldrüsen, im ruhenden Zustand eine ganz bestimmte chemische Zusammensetzung zukommt. Freilich ist es bisher nur in einzelnen besonderen Fällen möglich gewesen, diese Gesetzmässigkeit klarzulegen und die eigenartige chemische Zusammensetzung eines tierischen Gewebes, wie die eines Ge-

(29) Vgl. BUGGE, „Buch d. Grossen Chemiker", Bd. 2, S. 112.

steins, festzustellen". „Ich habe mich bemüht zu zeigen, dass die Biochemie zu Auffassungen und Betrachtungen kommen muss, welche den anatomischen analog sind, und ich glaube, dass die Darlegung dieser Bestrebungen die Bezeichnung „deskriptive Biochemie" rechtfertigt". (30).

Demgegenüber klingt es wie eine Stimme aus dem frühen neunzehnten Jahrhundert, wenn HANS DRIESCH, der Philosoph des neuen Vitalismus, sagt : „... Materie ist nicht einmal in *irgend* einem Sinne die Grundlage des Lebens. Entelechie ist von Materie total verschieden und der Kausalität der Materie durchaus entgegengesetzt" (31). Wäre es anders, so wäre die Entelechie so etwas wie ein chemischer Stoff, und dann müsste ja der lächerliche Vorgang möglich sein, „6 Kilo Löwensubstanz oder ... 3 Gramm Regenwurmsubstanz" als chemische Verbindungen eines Tages auf den Markt zu bringen, nämlich eine wägbare Menge „von derjenigen *homogenen* chemischen Substanz, welche das „ein Löwe sein" darstellen soll". Und trotzdem muss auch DRIESCH die Möglichkeit offen lassen, „dass bestimmte chemische Verbindungen in der Tat für die Besonderheit der organischen Formen im Sinne notwendiger formbildender Mittel existieren..." (32).

Gerade hier beginnt das metaphysische Problem zu einem wissenschaftlich und experimentell bearbeitbaren zu werden, gerade dies ist genau unsere Frage, und hier schweigt unser Autor. Gewiss kann man ihm daraus einen Vorwurf nur in sehr beschränktem Umfange machen; denn was hier an Möglichkeiten zu exakten Antworten vorliegt, beginnt erst in den letzten Jahrzehnten in unserer neuen Biochemie zu entstehen. Erst seit kurzem wissen wir einiges über die Wirkung von Vitaminen und Hormonen; wenn auch nicht die homogene Substanz für das „ein Löwe sein", so haben wir doch reine chemische Stoffe, die das ein Männchen- oder ein Weibchen-Sein bedingen. In wundervollen Untersuchungen ist gezeigt worden, wie einige der sichtbaren Eigenschaften bei einigen Pflanzen und Tieren abhängen, zwar nicht von der Wirkung einer chemisch isolierten Substanz,

(30) A. KOSSEL, „Die Probleme der Biochemie", Heidelberg, 1906, S. 8.

(31) H. DRIESCH, „Philosophie des Organischen", 2. Aufl., Leipzig, 1921, S. 510.

(32) Ebenda, S. 505.

aber doch von Form und Verteilung der Chromosomen. Von manchen Flüssigkeiten aus kranken Tieren, vor allem vom Blutserum kennen wir jetzt spezifische Reaktionen mit Lösungen vom Natriumvanadat, an denen man spektrographisch gewisse Krankheitszustände unterscheiden kann, z.B. Krebs von Tuberkulose (33). Die Gegenwart von Vitaminen beeinflusst die Form, in der die Knochen wachsen. Durch Verfüttern von Fluoriden kann man die Form der Zähne bei Ratten beeinflussen (34). Das sind einzelne der hervorragenden neuen Erkenntnisse zu unserm Problem; aber die Liste der auch jetzt noch offen gebliebenen Fragen wäre um ein Vielfaches länger.

Geschichtliche Betrachtungen pflegen im allgemeinen die Einleitung zu wissenschaftlichen Abhandlungen zu bilden. Alles Voranstehende ist auch nur Einleitung. Die Grundauffassung des Verhältnisses von Stoff zu Form bei Organismen kann in einer grossen Allgemeinheit gesehen werden, deren Umfang grösser ist, als die unserer exakten Wissenschaft zugänglichen Gebiete es sind. Aber eine Fülle von Einzelaufgaben ist erkennbar und lösbar. Diese Einzelaufgaben brauchen nicht unbedingt unter dem Gesichtspunkt jenes allgemeinen Problems zu erscheinen; vielmehr gibt es hierfür noch eine andere, geradezu entgegengesetzte Einstellung, eine solche nämlich, die praktische technische Ziele vor Augen hat. Man bemüht sich seit einigen Jahren, in beliebigen Formen herstellbare Kunstmassen, Kunsthaare als neue Faserstoffe, Kunsthäute als zusammenhängende Filmüberzüge herzustellen. Wir gehen damit ganz andere Wege als die Organismen bei der Erzeugung ihrer Haare und Häute, wir benutzen auch ganz andere Stoffe als die Natur für diese Zwecke. Aber es wäre nicht unmöglich, dass durch ein weiteres Studium der Naturgebilde neue Anregungen für diese wirtschaftlich ausserordentlich bedeutsamen Industrien gewonnen werden könnten.

(Heidelberg.) EDUARD FÄRBER.

(33) Vgl. „Naturwiss.", 1931, S. 940 ff.
(34) Vgl. „Industrial Eng. Chemistry", Januar 1932.

The Discovery of Mosaic Gold

Aurum musivum (mosaic gold; crystalline stannic sulphide, SnS_2).

KOPP *(Geschichte der Chemie*, iv, 130) was able to say regarding the discovery of this pigment : " über die Entdeckung des Musivgoldes sind mir überhaupt keine genaueren Angaben bekannt." I may, therefore, be permitted to draw attention to what seems to be the earliest known recipe for its preparation in a XIV cent. MS. of Italian origin, De Arte Illuminandi (as so described in the Naples Public Library Catalogue : it is without title) which has been published several times since 1877 (L'Arte della miniatura nel secolo XIV, codice della Biblioteca nazionale di Napoli, messo à stampa per cura di DEMETRIO SALAZARO, Napoli, 1877, 4to : I have not seen this). In the edition of LECOY DE LA MARCHE (Mémoires de la Société nationale des Antiquaires de France, 1888, xlvii, 248-286; p. 258; French translation by LOUIS DIMIER, L'Art d'Enluminure, Paris, 1927, 46, with reference to the well known XV cent. LEBEGUE MS. of Experimenta in Paris, published by Mrs. MERRIFIELD, Original Treatises on the Arts of Painting, 2 vols., London, 1849) the recipe reads :

de purpureo colore... qui vocatur aurum musicum *(sic)*. Recipe stagni partem unam, et funde, et proice super eo partem unam argenti vivi puri, et statim depone de igne, et tere cum aceto et modico sale communi, et lava cum aqua clara calida vel frigida, donec exeat aqua clara et sine sale, et deinde iterum funde materiam in igne, et pone super marmore; et postmodum recipe sulphuris vivi mundi et puri sicut ambram, partem unam et salis armoniac partem unam et contere peroptime, et totum incorpora simul cum supradicto mercurio et stagno, donec tantum nigrescat sicut carbo et si bene incorporatum. Deinde habeas unam vas vitri ad modum ampulle cum largo et brevi collo, ita quod vas sit ita magnum quod, posita intus materia, medietus sit vacua ad

minus [*such that the empty part is at least half*] : quo vas debetur bene lutari de bona argilla, bene speciata cum stercore asinino et cum cimatura pannorum [*waste draper's stuff:* Du Cange, *s. v.* cimare] ad spixitudinem unius digiti; et vas debet tantum esse lutarum quantum materia tenet [*only the part occupied by the material*]. Et posita intus materia supradicta, loca eum in furnello cum pila farata tantum quantum sit partem ampulle lutate capax, et obtura juncturas intus et pilam que est in furnello cum cineribus madefactio cum aqua. Et suptus accende imprimis ignem debilem de lignis salicis sive de candis vel hujusmodi, fortificando ignem usque ad VIIII horas, vel plus aut minus, usque ad signum inferius descriptum. Et vas debet esse coopertum cum una tegula liberta, ita quod poxit [possit] amoveri et reponi ad nutum. Et primo videbitur fumus niger, deinde albus, postea mistus. Et sepe mictatur intus baculis unus siccus et mundus hoc est in vase ubi est materia, ita quod non tangat materiam, et semper paulative vigoretur ignis, donec videantur in baculo scintille auree : et tunc dimicatur ignis, quia factum est, et infrigidato vase fracto recipiatur materia aurea, et servetur; Deo gracias.

A XV cent. Bologna MS. gives several recipes for the preparation of this purpurino (Merrifield, i, p. xcix; ii, 458 f., 470). The first one gives only mercury and Venetian tin, and is obviously incomplete. The others specify mercury, tin (including Roman tin), sulphur and salammoniac in varying proportions. Three specify equal weights, the others :

Tin	Mercury	Sulphur	Salammoniac
1	1	1½	1
2 oz.	1 lb.	(omitted)	2 oz.

The tin and mercury are first melted together, then ground with the other materials and the whole heated in a glass bottle with a very low neck, luted up the neck with lute of the philosophers and heated over a charcoal fire till smoke appears, when the fire is continued either till a silvery line appears (? subliming mercury) or the smoke comes off yellow, or the smoke ceases to come off. The recipes are much less clear than the one in the Naples MS.

The recipe in the Lebegue MS. (Merrifield, i, 54) specifies equal weights of tin, mercury, sulphur and salammoniac, first

makes tin amalgam, then grinds with the other materials and heats in a glass flask luted with clay (creta grossa). Details of heating are given and the recipe is clear and practical.

A recipe is also given imperfectly by CENNINO (who does not usually give directions for the preparation of the pigments he describes) (A Treatise on Painting, translated from TAMBRONI's Italian text of 1821 by Mrs. MERRIFIELD, London, 1844 : the original treatise was written in 1437: p. 159): "A gold called porporina. Take salt orminiaco, tin, sulphur and quicksilver, of each equal parts, except that there must be less quicksilver. Put these things in a vessel of iron, copper [obviously CENNINO never tried the experiment] or glass, melt the ingredients on tbe fire and it is done."

The preparation of mosaic gold was investigated by PETER WOULFE in 1771 (*Phil. Trans.*, lxi, 114) : the best proportions were 12 tin, 7 sulphur, 3 salammoniac and 3 mercury. The correct temperature in preparation is very important. In Germany it was little used, the cheaper powdered brass or tombac taking its place in the gold paint of Nuremberg (WIEGLEB, Geschichte des Wachsthums und der Erfindungen in der Chemie in der neuern Zeit, 2. Band, Berlin and Stettin, 1791, III).

It seems probable that the preparation was gradually evolved in the modification of the method of preparing artificial cinnabar (mercuric sulphide). The preparation from mercury and sulphur is already known to ψ-DEMOKRITOS (Syriac text : BERTHELOT, Chimie du moyen âge, II, 31, 77 ; Collection des alchimistes grecs, III, 39, 367), perhaps from Chinese sources (JOHNSON, Chinese alchemy, 1928, 79, 84, 106, etc.). The expensive mercury was perhaps partly replaced by tin, and the " spirit " sulphur partly by another " spirit," salammoniac. The Arabic chemists, such as JABIR and RHASES, classed sulphur, arsenic, mercury and salammoniac as spirits (HOLMYARD, Chemistry to Dalton, 1925, 19, 21), the last substance being merely added to the three " spirits " (= volatile substances) in the old classification of ZOSIMOS (BERTHELOT, Collection des alchimistes grecs, traduct., 152). An interesting intermediate recipe, in which tin is omitted, is given by LIBAVIUS (Alchymia, 1606, 86) from " an old book which came from Venice 200 years before PARACELSUS." In this a " blue cinnabar " (? azur, which meant red, not blue, in early

authors; a name for cinnabar itself : BERTHELOT, Chimie du moyen âge, I, 82) is made by subliming 4 parts of mercury, 2 of sulphur and 1 of salammoniac in a luted aludel until a blue vapour appears. AGRICOLA, he says, also describes it, but uses 3 of mercury, 2 of sulphur and 1 of salammoniac (De natura fossilium, IX; Opera, Basiliae, 1657, 652). Several similar recipes are given by ALBERTUS MAGNUS (De alchemia, Theatrum Chemicum, 1659, ii, 437 : there seems no reason to doubt the authenticity of this work) and GEBER (Summa perfect., i, 28; 1545 ed., p. 48) mentions a blue colour obtained from mercury. As WIEGLEB (*op. cit.*, 1792, i, 227) suggests, these recipes are probably from an Arabic source, and the same origin may be suggested for the preparation of stannic sulphide. Sufficient has, at least, been said to carry the matter further back than the German " Kunstbüchlein " of 1535 quoted by LIPPMANN (Alchemie, ii, 228; I have not seen the publication of DARMSTAEDTER he mentions). The preparation was much simplified by PELLETIER in 1792 (Mémoires et Observations de Chimie, tome ii, 1798, 87).

(London). J. R. PARTINGTON.

Notes and Correspondence

Query no. 38.—*Jean Trenchant, French mathematician of the second half of the sixteenth century.*

In the preface to his first book, the *Tafeln van interest* (Antwerp, 1582), SIMON STEVIN declares that such tables were first described by JEAN TRENCHANT, who gave a specimen of them in his *Arithmétique*. This work was first published in Lyon, 1558. A copy of the original edition exists in the library of Trinity College in Dublin, to whose courtesy I owe the facsimile of the title page herewith published. It bears the signature of the astronomer SAMUEL MOLYNEUX (1689-1728); see facsimile. I do not know of any other copy. It was often reprinted, to wit in Lyon, 1561, 1563?, 1566, 1571, 1578, 1588, 1602, 1605, 1608?, 1610, 1618, 1631, 1643; in Paris, 1617; in Rouen, 1632, 1647, 1660, 1675? That is, it was printed at least 16 times within a century. Most of these editions are quoted by G. ENESTRÖM in *Bibliotheca Mathematica* (vol. 2, 356, 1901).

There is a copy of the edition of Lyon, 1571, in the British Museum; one of the edition of Lyon, 1578, in the Library of Congress; one of the edition of Lyon, 1618, in the Harvard Library. The latter is the only one I have seen with my own eyes. The title is almost the same as that of the first edition, to wit:

L'arithmétique de IEAN (sic) TRENCHANT. *Departie en trois Liures. Ensemble un petit discours des Changes. Auec l'art de calculer aux Getons.*

But with the following addition (which may have been taken from an earlier edition).

Reueuë & augmentee en ceste derniere edition tant de plusieurs regles & articles, par l'Autheur, que d'une Table de poids de vingt deux Prouinces, correspondans l'une à l'autre.

A Lyon. chez PIERRE RIGAUD, M.DC.XVIII (375 p., plus une planche, la Table des poids), 17 × 10.5 cm.

This late edition contains a reprint of what seems to be the original dedication dated "De Lyon ce 9. de Iuillet 1557," to Monseigneur de MANDELOT, seigneur de Passis,..., gouverneur et lieutenant général pour sa Majesté à Lyon, pays de Lyonnois, Forests & Beaujolois."

It is very strange that the author of a book which enjoyed so much popularity should be practically unknown. We do not even know where and when he was born and where and when he died. He flourished in Lyon in 1557 (date of the preface) and if the statement on the title page of the edition of 1618 is correct he was still living at that date presumably in or near Lyon. Could one of our French contributors obtain more information about him in the Lyonese archives or other

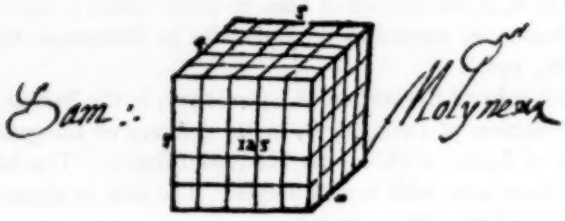
L'ARITMETI-
QVE DE IAN
TRENCHANT
DEPARTIE EN
TROYS LI-
VRES,

Enſemble

Vn petit diſcours des changes.

Auec

L'art de calculer aux Getons.



A LYON,
Par Ian Trenchant,

1558,

Auec Priuilege du Roy.

French archives? For a description of his *Arithmétique* see Father BOSMANS in the *Annales de la Société scientifique de Bruxelles* (33, 184-92, 1909).

STEVIN's statement as quoted by Father BOSMANS (*ibidem*, p. 186) raises another question. Says he : " I do not give these tables of interest as a personal invention ; I have simply developped those which already

exist. JEAN TRENCHANT has written before me on this subject in book 3, chapter 9, article 14 of his *Arithmétique*." But surely such tables must have existed even before 1558? Banking houses needed them badly; there must have been MS. tables of interest anterior to the middle of the sixteenth century, and if so, where are they now, and how and by whom were they compiled? GEORGE SARTON.

Query no. 39.—*Adriaan van Roomen's commentary on al-Khwārizmī* (c. 1598).

The great Dutch mathematician and physician ADRIAAN VAN ROOMEN —better known under his Latin name ADRIANUS ROMANUS (Louvain, 1561—Mainz 1615) had obtained an excellent manuscript of ROBERT OF CHESTER's translation of al-KHWĀRIZMĪ's algebra, thanks to the courtesy of his friend THADDEUS HAGEC of Prague. On the basis of that Ms. he began a commentary "*In* MAHUMEDIS *Arabis algebram*," of which we only know the prolegomena in the form of a printed fragment—72 pages in folio without title, preface and other introductory material—printed probably in 1598 or 1599, at Würzburg, where ROMANUS was then teaching, probably by GEORG FLEISCHMANN.

A copy of that interesting fragment existed before the war in the library of the University of Louvain. It was lost in the destruction of that library by the Germans when they invaded Belgium in 1914. Fortunately it had been examined and carefully described by Father HENRI BOSMANS. According to Father BOSMANS (1), another copy of it exists in the municipal library of Douay (Réserve C 203). There may still be other copies of it, which might be included in factitious volumes or miscellaneous collections?

For additional information see two very elaborate papers by Father HENRI BOSMANS: (1) Le fragment du commentaire d'ADRIEN ROMAIN sur l'algèbre de MAHUMED BEN MUSA EL CHOWÂREZMI (*Annales de la Société scientifique de Bruxelles*, 30, seconde partie, 267-79, 1902). (2) His notice on ROMANUS (*Biographie nationale de Belgique*, vol. 19, 848-88, 1907). Then, (3) LOUIS CHARLES KARPINSKI: ROBERT OF CHESTER's Latin translation of the algebra of al-KHOWARIZMI (164 p., New York, 1915; *Isis*, 4, 504-05); (4) SARTON: *Introduction* (vol. 1, 563-4; vol. 2, 175-7). GEORGE SARTON.

Query no. 40. — *Primitive tonsillotomy.*

In his interesting book "*On the trail of the veiled Tuareg*" (London, SEELY, SERVICE & Co., 1928, p. 231) DUGALD CAMPBELL mentions that these strange dwellers of the Sahara are acustomed to cut the tonsils

(1) In article SIMON STEVIN (*Biographie nationale de Belgique*, vol. 23, 1924).

of their babies soon after their birth. This custom is called *beli* in their own Tamachek language, and they call themselves (p. 265) *kel-tagilmuss*, the people of the veil. (1) This custom was entirely new to me. Has it ever been recorded elsewhere, investigated and explained?

GEORGE SARTON.

MISCELLANEA

Paris. Cours et conférences de l'Institut d'histoire des sciences (1933-34).—Section 1. P. DUCASSÉ : La géométrie d'AUG. COMTE et l'idée de philosophie scientifique. Section 2. F. MAROTTE : La création de la géométrie dans l'antiquité, envisagée dans ses relations avec l'enseignement.—P. BRUNET : Le système tourbillonnaire chez DESCARTES, MALEBRANCHE et leurs disciples.—GL. BERRIER : Introduction à l'histoire de la géodésie.—Section 3. HENRI VOLKRINGER : L'évolution des théories sur la nature de la lumière.—JEAN LOUIS DESTOUCHES : L'évolution des mécaniques ondulatoires.—JEAN MARIANI : L'idéal d'objectivation et l'histoire contemporaine de la physique théorique.—BRAUMAN : Les théories de l'affinité au XVIII^e siècle et dans la première moitié du XIX^e siècle.—Section 4. HÉLÈNE METZGER : La classification et l'hypothèse dans les doctrines chimiques du XVII^e siècle.—Section 5. DAUJAT : Les origines de la calorimétrie animale et de la bioénergétique.—PIVETEAU : Les données historiques et la portée philosophique de la discussion entre CUVIER et GEOFFROY SAINT-HILAIRE.—Section 6. GURWITSCH : Le développement historique de la Gestaltpsychologie. GEORGES LUTFALLA : Introduction à l'histoire de l'économie mathématique. CH. DE LA RONCIERE : La cartographie médiévale. — Le Sahara et l'Abyssinie au Moyen Age.—HENRI DEHERAIN : Histoire de l'Orientalisme français depuis le XVIII^e siècle. Les établissements d'enseignement et de recherches savantes.—Y. H. GOBLET : L'école cartographique et géographique anglaise de la fin du XVI^e et du commencement du XVII^e siècle.—Section 7. P. J. CHARLIAT : L'histoire de la marine marchande française du XVII^e au XX^e siècle. JEAN MARIE : Evolution de la Sécurité Maritime. LAIGNEL-LAVASTINE : Histoire résumée de la médecine française des origines à LAËNNEC.

Prix Binoux 1933.—Dans sa séance du 11 décembre 1933, l'Académie des sciences a décerné le prix Binoux à LOUIS PASTEUR-VALLERY-RADOT († 1933) pour sa contribution à la publication des *Œuvres* de PASTEUR (*Isis*, 5, 522) dont le dernier volume vient de paraître (L. G.).

(1) As opposed to their Muslim neighbors, their men are veiled and their women unveiled.

Reviews

Nicolae de Cusa *De Docta Ignorantia Libri Tres*. Ediderunt ERNESTUS HOFFMAN et RAYMUNDUS KLIBANSKY. (Opera omnia iussu et auctoritate Academiae litterarum Heidelbergensis ad codicum fidem edita. Vol. I.) 4, XIX+179 pp. Leipzig, MEINER, 1932. RM. 36.

The inadequacy of the existing editions of NICOLAS DE CUSA's works has become a byword with all who have engagedin a study of this important churchman and philosopher. This inadequacy—together with the neglect into which some of CUSANUS's works have consequently fallen—has given rise to diametrically opposed judgments as to his position in the history of science and philosophy. Such disagreement would seem to be the result of the lack of a comprehensive view of his works in their entirety based upon accurately established texts.

The Heidelberg Academy has outlined a plan for the publication of the philosophical and political works of CUSANUS in fourteen volumes over a period of eight years. Circumstances necessitate their limiting themselves to these for the present. They hope to complete their plan by the year 1939 which will mark the 500th anniversary of the initial publication of the *De Docta Ignorantia*. It is this work, in which CUSANUS set down in comprehensive manner the fundamental principles of his philosophy, that forms the first volume in the series.

The extreme importance of the work undertaken by the Academy may be demonstrated by one concrete example. In the course of his discussion of the various printed editions of the works of CUSANUS in his preface to the edition of the work here under discussion, HOFFMAN mentions the edition completed by PAUL ROTTA in 1913 only to dismiss it as being of no value in the determination of the correct readings of the texts of CUSANUS. He gives three reasons for his judgments: 1) ROTTA did not consult even one MS. of CUSANUS's works; 2) he committed numerous errors in the mere copying of the old texts printed in archaic characters; 3) he introduced far-fetched emendations and readings into his apparatus which he pretended to be a critical one. And yet we find that it was this very ROTTA edition that L. MOULINIER took as the basis for his French translation of the *De Docta* (Paris 1930),

though he tried to overcome the limitations of the former by collating it with two MSS. and with three of the four printed editions omitting, however, the earliest of 1498. It was the ROTTA edition, also, that ABEL REY in his introduction to MOULINIER's translation described as a " bonne édition critique."

HOFFMAN and KLIBANSKY, the editors of the *De Docta Ignorantia*, show a keen appreciation of the importance of the two fundamental steps mentioned above. They spared no effort in establishing an accurate text, for they collated every available MS.—fifteen in all—as well as all of the four printed editions. By carefully establishing the relationship of prototype and copy between the various MSS. they were able to make use of time and labor saving devices in recording the numerous variant readings. Though they made use of some of these devices also in the case of the printed editions, they were nevertheless careful to record even the least discrepancies between the readings of the printed texts, since, as they very aptly observe, it was the texts of these very editions that subsequent generations used as basis for their study and interpretation of CUSANUS's thought. Furthermore, in carrying the transmission of the text of the *De Docta* one step further, the editors took no unwarranted liberties with it. They preserved all the grammatical and stylistic peculiarities common to CUSANUS and his time, though they did do away with some of the difficulties arising from mere external orthographic deficiencies found in the printed editions.

The editors have set a standard of impeccability in their work which will fulfill the most exacting demands of the student of CUSANUS. Besides the apparatus in which they collate the fifteen MSS. and the four printed editions of the *De Docta*—omitting ROTTA's edition for the reasons given above—they have provided the text with two other apparatuses. In one they supply all references made directly or indirectly by CUSANUS to the various philosophers and theologians upon whom he drew for some of his views and discussions. In the other, they give full and clear references to the more important later authors who were indebted to or who mention CUSANUS. The determination of the sources and the investigation of the influence of CUSANUS as set forth in these two apparatuses were the special task of Mr. KLIBANSKY.

Besides the three apparatuses the editors have also included an index of names and an index of authors both prepared with an eye to thoroughness and usefulness. They have, so to speak, set up a well equipped laboratory in which the student of the *De Docta* may now work with an increased degree of accuracy and confidence.

HOFFMAN deals very briefly with the question of the sources and influence of CUSANUS. He takes the opportunity to announce the pro-

jected publication by KLIBANSKY of three most important and hitherto unpublished sources of CUSANUS. These are the *De sex dierum operibus* of THEODERICUS CARNOTENSIS, an anonymous commentary on BOETHIUS's *De Trinitate*, and various works by the disciples of the school of THEODERICUS CARNOTENSIS. KLIBANSKY will publish these as appendices to his work on the thought of the Carnotensian school. There also he will expound in detail the relationship between the *De Docta Ignorantia* of CUSANUS and the philosophical literature of his time.

S. KURLAND.

Christiaan Huygens (1629-95).—*Œuvres complètes* publiées par la Société hollandaise des sciences. Tomes I-XVII, La Haye, MARTINUS NIJHOFF, 1888-1932 (*à continuer*).

Nous avons grand plaisir a enregistrer dans *Isis* le progrès lent mais sûr de la publication des œuvres de HUYGENS. Cette édition fait le plus grand honneur à nos savants confrères de la Société hollandaise des sciences. Elle constitue une mine extrêmement riche pour l'histoire de la pensée scientifique au XVII^e^ siècle. Grâce à elle, et aussi aux admirables éditions de DESCARTES, FERMAT, PASCAL, TORRICELLI, GALILÉE, etc. qui l'ont précédée, notre connaissance du "grand siècle" devient vraiment aussi détaillée qu'étendue. A quand la synthèse? Et qui aura assez d'audace, d'intelligence, et d'énergie pour la tenter et y consacrer sa vie?

Nous avons rendu compte des Vols. I à XIV dans notre volume V, 154-6, 1923, et de courtes notes de notre Bibliographie critique ont annoncé les volumes suivants à mesure qu'ils paraissaient. A vrai dire pour une édition de cette envergure et de cette valeur des analyses détaillées sont à peine nécessaires, — ou plutôt on doit y renoncer parce qu'elles seraient interminables. Il suffit d'indiquer le progrès de l'œuvre entière et le contenu de chaque volume.

Rappelons que les vols. 1 à 10 (1888 à 1905) contiennent la correspondance de HUYGENS depuis 1638 (Aet. 9) jusqu'à sa mort en 1695 : une série de plus de trois mille lettres admirablement éditées.

Tome XI (1908). Travaux mathématiques, 1645-51 (*Isis*, 5, 155).

Tome XII (1910). Travaux de mathématiques pures, 1652-56 (*Isis*, 5, 155).

Tome XIII (2 vols., 1916). Dioptrique 1653, 1666, 1685-92 (*Isis*, 5, 155).

Tome XIV (1920). Calcul des probabilités. Travaux de mathématiques pures, 1655-66 (*Isis*, 5, 156).

Tome XV (1925). Observations astronomiques. Système de Saturne. Travaux astronomiques, 1658-66.

Recueil des observations (p. 55-163). *De Saturni luna observatio nova*,

1656 (avec traduction française, p. 172-77). Ce court pamphlet se termine par le fameux anagramme par lequel HUYGENS s'assurait la priorité dans la découverte de l'anneau. *Systema Saturnium*, 1659 (p. 209-38). Ceci inclut neuf appendices datés de 1658 à 1667. EUSTACHII DE DIVINIS SEPTEMPEDANI (1) *Brevis annotatio in systema Saturnium* (Rome, 1660), HUGENII *Brevis assertio systematis Saturnii sui* (Florence 1660), les deux textes avec traduction française et autres documents (p. 403-72). Observation de Saturne faite à la Bibliothèque du Roy en 1668 (*Journal des sçavans*, 11 février 1669) et documents y relatifs (p. 483-504). Détermination approximative de l'orbite d'un satellite de Saturne (1673), inédit (p. 509-12). Travaux astronomiques divers de 1658 à 1666 (p. 513-67).

Chaque texte est précédé d'un avertissement qui en indique l'origine, la portée, l'histoire. Ces introductions et les notes copieuses sont en partie appuyées sur les dix volumes de la correspondance ; elles prouvent combien les éditeurs ont été sages de publier d'abord cette correspondance sans laquelle l'édition scientifique des mémoires scientifiques eut été fort imparfaite. Excellents index.

Tome XVI (1929). Percussion. Question de l'existence et de la perceptibilité du mouvement absolu. Force centrifuge. Travaux divers de statique et de dynamique de 1659 à 1666 (*Isis*, 15, 238).

Tome XVII (1932). L'horloge à pendule de 1651 à 1666. Travaux divers de physique, de mécanique et de technique de 1650 à 1666. Traité des couronnes et des parhélies (1662 ou 1663).

L'Horologium oscillatorium parut à Paris en 1673, mais HUYGENS s'était occupé des horloges longtemps auparavant, et il n'était pas seul d'ailleurs. Le *Dialogo intorno ai due massimi sistemi del mondo* de GALILÉE énonçait les lois de mouvement des pendules — par ex. l'isochronisme des vibrations. Ce dialogue fut publié à Florence en 1632, mais il était déjà achevé en 1629 et MERSENNE avait fait connaître la découverte de l'isochronisme dès 1631. Depuis, le perfectionnement de l'horloge était à l'ordre du jour. Description d'une horloge à pendule construite pour HUYGENS en 1657 et qu'on peut voir aujourd'hui au Musée d'histoire de la science de Leiden. *Horologium*, La Haye 1658 (p. 41-84) avec traduction française et six documents complémentaires. Projet de 1659 d'une horloge à pendule conique. L'horloge à pendule à arcs cycloïdaux de décembre 1659 et son réglage par le poids curseur de 1661. Projet de 1660 d'une édition amplifiée de l'*Horologium* de 1658, suivi de quelques morceaux des années 1659-1666, correspondant à des sujets traités dans l'*Horologium* de 1673. Horloges marines et sympathie des horloges. Brève instruction

(1) EUSTACHIO DIVINI, born at San Severino, c. 1620; flourished in Rome, telescope-maker; still living in 1663.

au sujet de l'emploi des horloges pour trouver les longitudes. Publié en néerlandais en 1665 ; une traduction anglaise assez libre parut en 1669. Texte néerlandais et traduction française (p. 199-237). Des efforts similaires étaient faits en Angleterre, mais ils n'aboutirent pas : presque un siècle devait encore s'écouler avant que les horloges ne fussent suffisamment perfectionnées pour déplacer la méthode rivale des observations lunaires. (2)

Travaux divers de 1650 à 1666 : Percussion. Pesanteur. Taille des lentilles. Compression et raréfaction de l'air, machines pneumatiques, poids de l'air, etc. Modification de la machine à calcul de PASCAL. Expérience sur les sons d'une cloche. Réfraction de la lumière. Anneaux dits de NEWTON. Observation sur le magnétisme des briques et des ressorts d'acier des petites horloges. Thermométrie (toutes ces pièces sont inédites ; certaines sont fort courtes).

De coronis et parheliis. Traité que HUYGENS rédigea en néerlandais après de longues années d'études : il était prêt en 1667 mais ne fut pas imprimé. 's GRAVESANDE en fit paraître une traduction latine dans les *Opera reliqua* (vol. 2, 1728). Le texte original néerlandais paraît donc ici pour la première fois, avec une traduction française et des notes abondantes (p. 364-516).

Ce volume est orné d'un frontispice représentant l'église de Schéveningue d'après un dessin fait par HUYGENS le 29 juillet 1658 : un fort beau dessin, ma foi.

On ne saurait trop admirer cette publication modèle. Quand donc l'Angleterre nous donnera-t-elle une édition de NEWTON qui puisse lui être comparée ? L'exemple que les Hollandais nous offrent semble parfait à tous les égards, y compris celui de la lenteur. Car cette lenteur désarme les critiques qui seraient tentés de considérer une telle publication comme un luxe injustifié. C'est du luxe en effet, surtout dans une période de crise et de misère — mais le minimum de luxe que l'humanité se doit à elle-même. La dépense de ces dix-sept splendides volumes a été répartie sur quarante cinq années ; donc la dépense annuelle a été minime — et qu'importe un délai d'un demi siècle pour les immortels ?

Mon seul regret c'est que les éditeurs ne soient pas nommés. Cela ne me paraît pas juste. Tout travail un peu considérable doit nécessairement contenir des erreurs et des lacunes, et il vaudrait mieux que chaque collaborateur fut responsable pour sa part de la tâche commune, et put recevoir personnellement les louanges et les critiques méritées.

GEORGE SARTON.

---

(2) RUPERT T. GOULD, *The marine chronometer* (London, 1923, *Isis*, 6, 122-9). F. MARGUET, *Histoire générale de la navigation* (Paris, 1931; *Isis*, 19, 235-7).

Clifford Dobell.—*Antony van Leeuwenhoek and his "Little Animals,"* being some Account of the Father of Protozoology and Bacteriology and his multifarious Discoveries in these Disciplines, collected, translated, and edited, from his printed Works, unpublished Manuscripts and contemporary Records. Published on the 300th Anniversary of his Birth. VIII+435 p., 32 pls., 4 figs. in text, HARCOURT, BRACE & Co., New York, 1932. Price $ 7.50.

This up-to-date interpretation of the actual discoveries of this pioneer Dutch microscopist among microscopic organisms has the indispensable prerequisite that the interpreter is himself familiar with the organisms concerned, with their sources, and their behavior. He is therefore in a position to understand what LEEUWENHOEK in his curiously restricted vocabulary attempted to describe and what his translators into Latin and English, who presumably were unfamiliar with the strange little animals which he had discovered, were transcribing into other tongues. The *De Re Metallica* of AGRICOLA had many translators before a mining engineer and his helpmate with the assistance of Latin scholars made this mediaeval treatise on ancient mining intelligible to the engineers and scholars of today.

Mr. DOBELL has rendered a real service to the history of science by his scholarly treatise in English which provides critical translations of those parts of LEEUWENHOEK's writings which deal with Protozoa and Bacteria and discussions of mooted or obscure points which elucidate his conclusions. Another considerable service rendered is his ferreting out the facts regarding the life of this obscure draper and Chamberlain of Delft, and of revealing somewhat of the character of this inquisitive and secretive user of the microscope.

Among the useful contents of the book is a table of *Memorabilia*, in which the correct dates, all in new style, of the more significant events in the life of LEEUWENHOEK are recorded. The transition from the Julian to the Gregorian calendar was in progress, though not simultaneously in Holland (1582 in Delft) and England (1752), during LEEUWENHOEK's life.

Visitors to Delft have found a bronze effigy marking the place where LEEUWENHOEK was supposed to have lived and worked, on which the discovery of the Infusoria is noted with the date of 1676. Our biographer states that the site is incorrect and records the date of the discovery of the Infusoria as 1674.

LEEUWENHOEK had the spirit of the explorer, the creative imagination of the experimentalist, and the quantitative urge of the mathematician. These qualitites were combined with an inveterate and successfully maintained secretiveness and an itch for notoriety. The first named

qualities made him the discoverer of the Protozoa and the Bacteria, of Capillary circulation, of parthenogenesis in Aphids, and of budding in Hydra. They led to his search for animalcules in strange places and varied and seemingly absurd substances, to microscopical observations on insects, rotifers, vinegar eels, blood corpuscles, spermatozoa, and exploding gunpowder. He discovered animalcules in his microscopical search for the reason why pepper is hot!

As a scientist he lacked general concepts and any sense of the relations of his discoveries. His writings are tiresome because of repetition and discontinuity. He had more of a childish glee over the novelty of his discoveries than an appreciation of their significance.

DOBELL critically surveys in his *Envoy* LEEUWENHOEK's place in the history of science. He was not the father of micrography. BOREL, POWER, HOOKE, and MALPIGHI have at least as good or a better right to this title. He was not the inventor of the microscope, and it might also be said that he made no improvements in it. He used only a simple lens. He can hardly be called a father of biology, for his contributions are detailed observations rather than basic ideas. He has, however, some right to be called the father of cytology, histology, haematology, or the first milk-analyst, but preeminently the father of protozoology and bacteriology.

DOBELL's criticisms of LÖFFLER's and SINGERS' later support of KIRCHER's claims to the discovery of bacteria in rotting bodies seem rather overstressed than convincing. They are enveloped in a haze of dust stirred up about KIRCHER in general. He quotes KIRCHER (1658) as saying " all decomposing things swarm with an innumerable brood of worms invisible to the naked eye : which even I myself would never have believed, had I not proved it by repeated experiment over many years." He then remarks : " Now this passage contains no obvious reference to any organisms other than worms or insects—well known to everybody at the time when KIRCHER wrote... Any ordinary person would conclude that KIRCHER never described bacteria for the simple reason that he never saw them."

Now KIRCHER's microscopes were apparently as good as LEEUWENHOEK's simple lenses, though DOBELL states that " there is good reason to believe that KIRCHER possessed no instruments capable of showing any objects of the order of magnitude of common bacteria." Unfortunately what this good reason is remains unexplained. One has merely to accept the authority of the writer, a reprehensible practice on the part of KIRCHER, as DOBELL so clearly and forcibly demonstrates in his characterization of this Jesuit priest. KIRCHER's explicit statement in the passage quoted may be a fabric of lies, but DOBELL's heated assertions that

KIRCHER never saw bacteria seem to be inadequately supported.

The completeness of DOBELL's treatise is shown in his *Elucidations and Annotations*, which cover such subjects as LEEUWENHOEK's name, language, microscopes and methods, dwelling, draughtsmen, portraits, and seals. There is a brief list of LEEUWENHOEK manuscripts, and a bibliography of twenty-nine of his major publications. A bibliographical list of references and sources covers twenty-three pages annotated as to the author's opinion of the value or worthlessness of the contribution.

All students of the history of science will always be deeply indebted to the author of this painstaking and laborious study of a subject so important in relation to the newer disciplines of the biological sciences and of medicine and for the sympathetic presentation of this picturesque figure and his little animals.

April 19, 1933. CHARLES A. KOFOID.

Conway Zirkle.—*Some forgotten records of hybridization and sex in plants*, 1716-1739. Journal of Heredity (Organ of the American Genetic Association), Washington, D.C., November, 1932, vol. 23, 432-48, 3 portraits.

This short and unpretentious paper is a notable contribution to the history of botany and also to the history of science in England and in the American colonies in the first half of the eighteenth century.

It deals mainly with 1. COTTON MATHER (1663-1728), 2. THOMAS FAIRCHILD (1667?-1729), 3. PHILIP MILLER (1691-1771), 4. PAUL DUDLEY (1675-1751), 5. JAMES LOGAN (1674-1751), 6. JOHN BARTRAM (1699-1777). Portraits are given of 1, 4, and 5. It is a pity that the author did not include more information on the lives of these distinguished men, for botanists, even the American and English ones, may not know much about them. As to continental botanists it is probable that they know nothing of them. Notices on 1, 2, 3 and 5 may be found in the (English) *Dictionary of National Biography*; and notices on 4 and 6 in the *Dictionary of American Biography* (now in course of publication).

The botanical facts quoted by ZIRKLE concerning each man are not always new, but he has rendered a good service by arranging them in a new order and he has increased the significance of each by evidencing its relationships to all the others and showing it in its proper perspective. Thanks to him an important chapter of historical botany will have to be rewritten.

With the author's permission, I reproduce the chronology and summary, as printed at the end of his paper.

" 1676. GREW in an address before the Royal Society, recorded his own belief and that of Sir THOMAS MILLINGTON, that the stamens

are the male organs of the plant and that the pollen acts as vegetable sperm.

" 1694. CAMERARIUS in a letter to VALENTIN describes his experiments which proved that pollen is necessary for seed development in *Mercurialis*, *Morus*, *Rianus* and *Zea Mays*.

" 1703. MORELAND tried to discover how pollen influenced the ovules.

" 1714. GEOFFROY reported that the seeds of *Zea Mays* and *Mercurialis* are infertile when no pollen reaches the stigma.

" 1716. COTTON MATHER reported certain observations on *Zea Mays* and recorded (1) wind pollination, (2) hybridization (variety cross) and (3) the resemblance of some of the progeny to the male parent; he also reported a cross (?) between *Cucurbita Pepo* and *C. maxima* (?).

" 1717. BRADLEY removed the anthers from Tulips, which consequently produced no seed. He also noted spontaneous hybridization in several varieties of *Pyrus Malus* and *Auricula (Primula Auricula)*.

" 1717. FAIRCHILD crossed *Dianthus caryophyllus* with *D. barbatus* and found that the progeny resembled both parents. This was the first recorded artificial hybrid.

" 1721. MILLER described (1) several instances of spontaneous hybridization in *Brassica* and the resulting mixed and variable progeny; (2) insect pollination in Tulips; and (3) sexual reproduction in *Spinacio* and *Cucumis*.

" 1723. In a garden in Stenbrohuld, according to LINNAEUS, all male flowers were removed from a plant of *Cucurbita Pepo* which, consequently, bore no fruit.

" 1724. DUDLEY described wind pollination and variety crosses in *Zea Mays*.

" 1735. LINNAEUS commented on the artifices resorted to by gardeners to obtain hybrids in *Tulipa* and *Brassica*.

" 1735. LOGAN showed by controlled experiments that pollen is necessary for the formation of grain in *Zea Mays*.

" 1739. BARTRAM, in a letter to Col. WM. BYRD, states that he has crossed species (*Lychnis*), obtained strange hybrid forms and hoped to develop new horticultural varieties.

" 1749. GLEDITSCH fertilized a female palm tree (*Chamaerops humilis?*) in Berlin with pollen brought from a male tree in Leipzig, whereupon the palm produced fertile seed for the first time.

" 1750. HAARTMAN records forms which he assumes on taxonomic grounds to be natural hybrids in the genera *Veronica*, *Delphinium*, *Saponaria*, *Actaea*, etc.

" 1761. KOELREUTER systematically investigated hybridization and produced artificial hybrids in *Nicotiana*, *Kedmia*, *Dianthus*, *Matthiola*, *Hyoscyamus*, etc.

" Summary. A number of early records of plant hybridization made by English and American botanists have been unknown to the historians of the science, while others have been referred to in manner which minimized their importance. Certain other observations which helped to prove that plants reproduced sexually have been recorded, but mis-dated.

" 1. The first record we have of spontaneous hybridization is a letter written by COTTON MATHER on September 24, 1716, to JAMES PETIVER, F.R.S. The account was printed by MATHER in *Religio Philosophica: or The Christian philosopher*, London, 1721. He described variety crossing in *Zea Mays* and the pollination of squash by gourds.

" 2. The first recorded artificial hybrid resulted from a cross, *Dianthus caryophyllus* × *D. barbatus*. The investigator who made this cross should be known as Mr. THOMAS FAIRCHILD, of Hoxton, not as ' a gardener of London.' FAIRCHILD's hybrid is described by BRADLEY (1717), MILLER (1731), COLLINSON (1740), and DARWIN (1781).

" 3. PHILIP MILLER was the first to describe insect pollination (in tulips). The date of his observation was neither 1751 (SACHS) nor 1731 (GREEN), but was at least as early as 1721. In that year he wrote to BRADLEY (Oct. 6) and BLAIR (Oct. 19 and Nov. 11) and described not only insect pollination in tulips and sexual reproduction in spinach, cucumbers and melons, but also spontaneous hybridization in cabbage when different varieties were grown near one another.

" 4. PAUL DUDLEY in 1724 described wind pollination and variety crosses in *Zea Mays*.

" 5. JAMES LOGAN in a letter to PETER COLLINSON dated November 20, 1735, described his famous experiments on pollination in *Zea Mays*. He later published the results of his investigations in Leiden (1739) and London (1747).

" 6. JOHN BARTRAM stated in a letter written in 1739 to Col. WM. BYRD that he had crossed species within a genus and obtained a mixed and variable hybrid progeny. An undated manuscript in BARTRAM's handwriting contains an account of hybridization in *Lychnis*."

The author is preparing a second paper on the same subject, containing many additional records of hybridization of the first half of the eighteenth century. We shall speak of it in *Isis* as soon as it appears.

GEORGE SARTON.

William Arthur Heidel.—*The heroic age of science.* The conception,

ideals, and methods of science among the ancient Greeks. VII+ 203 p. Baltimore, WILLIAMS & WILKINS, 1933 ($ 2.50).

Judging from its title one would think that this book deals with early science. This first impression would seem to be confirmed by the epigraph taken from WHEWELL's *History of the inductive sciences* (vol. 1, 65, 1837):

"The sages of early Greece form the heroic age of science. Like the first navigators in their own mythology, they boldly ventured their untried bark in a distant and arduous voyage, urged on by hopes of a supernatural success; and though they missed the imaginary golden prize which they sought, they unlocked the gates of distant regions, and opened the seas to the keels of the thousands of adventurers, who, in succeeding times, sailed to and fro, to the infinite increase of the mental treasures of mankind."

Having referred to WHEWELL I found to my horror that when he spoke of "the sages of early Greece" he meant not only the early natural philosophers but ARISTOTLE as well! WHEWELL may be excused, for he wrote almost a century ago, but since his time the history of ancient philosophy as well as the history of ancient science have been renewed from top to bottom. Professor HEIDEL is a sort of philosophical RIP VAN WINKLE, who read WHEWELL's book a long time ago, fell asleep over it, then after a long interval woke up and produced something "new" which is a good deal worse than the old. It should be noted than when HEIDEL first studied WHEWELL's book the latter was already obsolete in many respects, but as it handled with some authority various subjects of which HEIDEL could not understand all the implications he had no means of using it critically.

HEIDEL's book does not deal with the beginning of Greek science, but with almost the whole evolution of Greek and Hellenistic science, —from THALES to GALEN,—and this without cautioning the reader in any way or giving him any chronological bearings. To realize the implications of this it will suffice to bear in mind that between THALES and GALEN more than seven and a half centuries elapsed, and that that very long period is not covered by a single development in a single country, but by sundry independent developments in three continents. The most elementary history of Greek science would distinguish in it at least four periods radically different in almost every respect: the early or "heroic" age, during which the results of Oriental science were assimilated and the fundamental problems of science formulated; the classic or golden age (c. 450 to c. 300 B.C.); the Alexandrian or Hellenistic age; and finally the Roman age. The distance from THALES to GALEN is as great as the distance between a scientist of the second half of the twelfth century and ourselves, and the differences are equally complex

in both cases. The author remarks (on p. 61) " Greek science has not, as a rule, been fortunate in either its assailants or its champions. Too often they have shown themselves wanting in the historical sense which is indispensable in setting a true value upon anything that belongs to the past." I do not know which authors HEIDEL had in mind, but it is certain that none has ever been as wanting in historical sense as himself, for throughout his book he continually mixes up all the centuries without any warning to the reader. By the way, one wonders for which class of readers this book is intended : it is far too vague and sketchy for scholars, and the lack of chronological order and of clearness makes it highly unsuitable for beginners. The book is hard enough to read for one who knows the history of Greek science; for one who does not know it very well (that is, the majority of readers) the book is hopelessly confusing, and the poor student will be far worse off after having read it than before.

The author quotes frequently STUART MILL, WILLIAM JAMES, and old authorities, but there is no indication that he has studied the recent books which have fundamentally renewed our knowledge of ancient thought. No trace of GOMPERZ, TANNERY, ZEUTHEN, MILHAUD, LORIA, etc. All these men wrote while RIP VAN WINKLE was asleep and he did not bother to consult their memoirs wherein almost every problem of Greek science has been reconsidered and explained in a new way. He hides his intellectual laziness behind cryptic phrases such as this one (p. 31) : " The answer must for the present be given in general terms, because to attempt to prove the correctness of the writer's position by citation of chapter and verse would be out of place, though the conclusions are the result of many years devoted to the study of the problem," or poor alibis of this kind (p. 195) " Controversial questions have been purposely avoided, and therefore many things which might have been mentioned were excluded." Goodness ! Controversial questions touch more often than not the very core of a subject, and this is especially true of the present one, but the author has accomplished the extraordinary feat of writing two hundred pages on Greek science without even mentioning some of the fundamental problems of Greek mathematics, physics, astronomy, and biology ! I do not remember having seen in it a single reference to the problem of irrationals which created a deep intellectual crisis and affected the development not only of mathematics but also of philosophy. In his brief account of Aristotelian biology he manages to miss the main points.

Speaking of the lever HEIDEL says (p. 62) : " Let us suppose that an archaeologist discovers balances with both equal and unequal arms —it is then evident that the principles of the lever were at least empirically

known to those who constructed the balances. If these are marked with a graduated scale, and if the weights are standardized, one must conclude that the theory also was known." This is obviously wrong. After having obtained a series of equal weights by means of a balance with equal arms, it would be easy enough to graduate any lever empirically, without any thought of, or interest in, the general theory of the lever. However, we happen to have such a theory composed by ARCHIMEDES (1), and though modern logicians may find flaws in it, (2) it is one of the most glorious achievements of the Hellenistic age.

One of the most fascinating subjects in the study of ancient science is the transition from the pre-Homeric oriental science to the beginning of Greek science. Within the last twenty-five years our knowledge of Egyptian and Mesopotamian science has increased considerably (the readers of *Isis* are well aware of this for there is scarcely a volume or even a number which does not bring some new information on the subject). Almost every one of the numerous excavations conducted all over the Near East brings additional proof of the interrelations between the Greek and Oriental centers of culture. For a great many scientific topics we are now able to trace the oriental antecedents. In short, it has become inexcusable to speak of Greek mathematics, astronomy, physics, chemistry, medicine without some sort of oriental introduction. Unfortunately all that information was accumulated while RIP VAN WINKLE was asleep and when he woke up he was far too lazy to try to understand what happened during his slumber. It is true he went to the trouble of travelling to the Near East, but what he did or saw there must remain a mystery; he must have been half asleep still—or unable to see anything clearly because of his prejudices and incurable muddleheadedness. Instead of studying the abundant evidence already available on the oriental origins of Greek science, HEIDEL prefers to dismiss the question in the following terms which are very characteristic of his general attitude (p. 198): "The impression one gets from a study of Greek science is certainly that it is essentially independent of external influences although the eager Greek took his information about matters of fact wherever he found it. He certainly did not conceal his debt to the Orient; on the contrary, there is every reason to suspect that in many cases he invented a theory of borrowing from abroad, where there was none in fact." (!?) It would have been more honest to state "I have not studied the matter and do not want to study it."

The final chapter on "experimentation" proves that the author has

(1) Not quoted by HEIDEL. According to the index ARCHIMEDES is mentioned only twice in the whole book, EUDOXOS twice, EUCLID and APOLLONIOS not at all!

(2) V. F. LENZEN: ARCHIMEDES' theory of the lever (*Isis*, 17, 288-89, 1932).

no clear conception of the experimental method and of the experimental spirit of modern science. He speaks of these matters like an outsider, who is unable to reach their very heart. Consider, e.g., this sentence (in another chapter, p. 66): "When one contrasts Greek and modern science in regard to method the difference lies, almost entirely, in the greater care which the modern scientist is expected to exercise in determining the precise nature of his data. It is at this point that the 'experimental method,' of which one hears so much [!?], has demonstrated its supreme value. Perhaps one may say that fundamentally this difference between ancient and modern science is due to the greater seriousness with which it is now pursued."...The fact is that in spite of a few examples of good experimental research (3) the Greeks failed altogether to understand the logical structure of the experimental method. This is very astonishing considering their genius in other matters not less difficult, but so it is.

To conclude, this book on Greek science is utterly insufficient in every respect: historic, philosophic, and scientific. Instead of clarifying a difficult and beautiful subject, it befogs it and makes it more difficult and less beautiful; instead of advancing it, it puts it back. It is one of the worst books which it has been my duty to read within the last twenty years, and unfortunately the evil which it has created can never be entirely undone. GEORGE SARTON.

Abel Rey.—*La jeunesse de la science grecque.* XVII+537 p. (Bibliothèque de synthèse historique. L'évolution de l'humanité. La science dans l'antiquité, 2). Paris, La Renaissance du Livre, 1933 (40 fr.).

It is a great relief to turn from the obscurity and humbug of HEIDEL's book to this treatise wherein our present knowledge of the heroic age of Greek science (i.e., down to the middle of the fifth century B.C.) is set forth with French clearness. The author, professor of the history and philosophy of science at the Sorbonne, was admirably prepared for the writing of it by his elaborate studies on pre-Hellenic science, the results of which appeared in various memoirs and in a general synthesis "*La science orientale avant les Grecs*," Paris 1930, (1) of which the present volume is the continuation (a third volume will deal with Greek science from the middle of the fifth century to the end of the Hellenistic age). Indeed in spite of the fact that our knowledge of early oriental science

(3) The best example is perhaps GALEN's physiological experiments, to which, I am glad to say, HEIDEL refers on p. 180.

(1) It was not reviewed in *Isis*, no review copy having been received. See review by O. NEUGEBAUER in *DLZ* (1930, 2060-62).

is still very incomplete, it is already sufficient to make us appreciate how much the Greeks borrowed from their oriental predecessors, and hence it is no longer permissible to discuss Greek science without taking the Oriental origins into account.

REY's synthesis of early Greek thought is the first to be built deliberately on Oriental foundations, and even if those foundations are not as sound as specialists like NEUGEBAUER would wish, we must give our Parisian colleague full credit for this bold initiative. I shall not attempt to examine the details of his work, which is meant for the educated public rather than for scholars. (2) It seems more worth while to describe its composition.

It is divided into four books, of which I shall indicate the main subdivisions.

I. *Beginnings of Greek science*. School of Miletos (95 p.). THALES. ANAXIMANDER. ANAXIMENES.

II. *Evolution of Greek thought around Pythagorism* (79 p.). PYTHAGORAS and his school. XENOPHANES. PARMENIDES. ZENO OF ELEA.

III. *Mathematics and physics from the end of the sixth century to the middle of the fifth* (215 p., this being by far the longest part). This includes a chapter on the geometry of ĀPASTAMBA (p. 257-69).

IV. *Technical progress to the middle of the fifth century*. (122 p.) Astronomy and cosmology. Technical physics. Medecine and biological theories (17 p.). Geography, geology, history (8 p.).

This plan is comprehensive and rational. The title is very well chosen: the youth of Greek science is a better description of that marvelous period than the heroic age,—youth, such youth at least, is heroic to be sure, but heroism is not always youthful. "La science hellène, si on l'étudie avant l'époque de SOCRATE, de DÉMOCRITE et de PLATON, en s'arrêtant à l'apogée de la grande civilisation classique, a tout de la jeunesse : l'enthousiasme, la hardiesse, la curiosité ardente, la recherche dans tous les sens." (p. 503). According to the author the main characteristics of that period are : 1. Awakening of the experimental science in a broad way; that is, physics ceases to be mythical. 2. Development of the mathematical method in its geometrical form. 3. Development of the inductive-mathematical method (a combination of the first two). 4. Development of the method of logical deduction.

The final paragraph of the author's conclusion may be quoted. It will give our readers a good idea of his style and attitude. Summing

(2) There are abundant references in the footnotes to the original texts (e.g., DIELS and BURNET) but few to other studies. The general bibliography given at the end of the book, is incomplete and somewhat erratic.

up the intellectual situation at about the middle of the fifth century B.C., he says (p. 519):

"N'oublions pas, au point de vue technique, que nous arrivons à la période où les démocraties grecques s'administrent par le discours, où les historiens analysent ces discours, ces articulations de 'motifs,' qui ont entraîné la persuasion, et que la Sophistique, au premier rang dans la génération qui vient, commence et fleurit déjà à la fin de notre période. Toute cette technique réagit sur la recherche scientifique. Elle va en former un des éléments primordiaux, voire bientôt l'élément primordial. Elle retrouve dans l'ambiance spirituelle les vieilles traditions mystiques sur la puissance du mot, de certains mots, qu'il faut connaître et prononcer, sur la puissance des formules (assemblages de mots, tout comme les calculs sont des assemblages de nombres) dont il faut connaître le secret agencement. Nous pensons toucher là un point sensible de l'esprit grec. Nous voyons autour de l'Eléatisme une mystique du discours, du logos (qui signifie aussi calcul), une mystique du rationnel, tout comme autour du Pythagorisme une mystique du nombre, et autour de l'Ionisme une mystique de la nature. Ces trois mystiques jointes aux techniques qui les positivent et les laïcisent, ne nous ouvrent-elles pas une porte par où apparaît en perspective l'exaltation de la pensée scientifique dans ce qu'on a appelé le miracle grec, dans ce que nous appellerons : la jeunesse de la Science grecque, et tout uniment de la Science?"

We are looking forward to the publication of Professor REY's third volume with genuine impatience. His history of science in ancient times is the first of its kind and scale; it will be for scholar and teacher alike a valuable tool and will encourage further progress in this attractive field of research. GEORGE SARTON.

The Macdonald presentation volume.—*A tribute to Duncan Black Macdonald, consisting of articles by former students, presented to him on his seventieth birthday*, April 9, 1933. x+487 p., portrait and illustrations. Princeton, University Press, 1933. ($ 6).

The publication of this beautiful *Festschrift* will give great pleasure to Dr. MACDONALD's many friends at home and in distant parts of the world, and they will derive from it considerable information on various Islamic and other subjects. It contains no less than 29 papers, plus a biography of Dr. MACDONALD, a bibliography of Dr. MACDONALD's writings and an excellent portrait.

According to methods followed in *Isis*, the papers of special interest to our readers are not mentioned here but they will be registered in our Critical Bibliography, each paper in its proper place. Thanks to this procedure these papers will be brought to the attention of every scholar interested in their subject matter.

The purpose of the present note is simply to announce the publication of the book, to congratulate the editorial committee headed by Professor

WILLIAM G. SHELLABEAR, and to express once more our best wishes to the great scholar and man, my revered *murshid*, to whom this volume was presented.

At a time when relatively few scholars understood the meaning of the History of Science, Dr. MACDONALD showed the keenest interest in our Society and in *Isis*. He is one of the associate editors of *Isis* and has contributed a very important paper (1) and many reviews to it. May his work and influence continue for many years to come for the benefit and the joy of his pupils and friends! GEORGE SARTON.

Reginald S. Clay and **Thomas H. Court.**—*The History of the Microscope*, compiled from original Instruments and Documents, up to the Introduction of the achromatic Microscope. XIV+266 p., 164 figs. in text. CHAS. GRIFFIN & Co., London, 1932. £ 1-10-0.

This work is an abundantly illustrated technical account of the evolution of the compound microscope from its invention in 1590 by the JANSSENS in Holland up to about 1845, shortly after which the improvement in the systems of lenses used created the achromatic optical equipment of this instrument of research. The authors have utilized the instruments of the collection made by the late Sir FRANK CRISP, Hon. Sec'y. of the Royal Microscopical Society of London, the National Collection of the Science Museum at South Kensington, including the Court Collection now in that museum, those of the Deutsches Museum at Munich, of the Museum at Kassel, of the Conservatoire National des Arts et Métiers of Paris, and of the collection of Professor CROMMELIN of Leiden. The authors are concerned more with the general pattern, the mechanical parts, and the accessories, rather than with the optical parts and the formulae for the lens systems, largely for the simple reason that the evolution of these parts was slow and of little significance. The work consists of a succint review of the history of magnifiers, including the contributions of ALHAZEN, VITELLO, ROGER BACON, DELLA SPINA, DIGGES, BOURNE, METIUS, LIPPERHEY, the JANSSENS, BOREEL, BONANNI, FONTANA, DREBBEL, HUYGENS, GALILEO, DESCARTES, KIRCHER, and SCHOTT. A second chapter is devoted to the microscopes of ROBERT HOOKE, author of the "Micrographia" (1665) as used both at home and abroad.

Though the compound microscope, owing to its greater convenience, due to its larger working distance and its comparative ease of manipulation, was to be preferred for ordinary observations, the greater part of the more critical work down to the early part of last century was carried

(1) Continuous re-creation and atomic time in Moslem scholastic theology (9. 326-44. 1927).

out by means of the simple microscope, and many of the most important discoveries of the early microscopists were made by its aid. The microscopes used by BONANNI for the plates of his "*Micrographia Curiosa*" (1691) and these described by KIRCHER in his "*Ars Magna et Umbrae*" (1696) were simple magnifying glasses, as was also that used and made famous by LEEUWENHOEK.

It was the revelations of these simple lenses which formed the basis for his famous reports to the Royal Society of London from 1673 on. The same simple optical equipment served for the more elaborate mountings of the instruments of HARTSOEKER, MUSSCHENBROEK (used by SWAMMERDAM), and CULPEPER. The screw-barrel microscope was mounted by the Italian TORTONA and was utilized and later described by Lord CAMPINI at Rome (1689), used by HARTSOEKER, BONANNI, and presumably by MELLIN, whose lenses were sent by order of the Royal Society to MALPHIGI. The name of WILSON, the London instrument maker, is widely associated with this screw-barrel type of microscope, although he was not its inventor.

Although many mechanical improvements tending to make the microscope more convenient to handle were continually being introduced by the makers both in England and abroad, both to the single microscope and also still more to the compound microscope, very little real improvement was achieved in the optical system from the time of HOOKE up to the end of the 18th century. DIVINI, GRINDL, JOBLOT, ADAMS, and possibly others, used lenses in pairs both in the eye-piece and even for the objective, but none of these makers made any definite attempt to correct the aberration. MARTIN placed an extra convex lens at the top of the snout, and this was a real improvement in the optical system of the compound microscope, for it was an attempt to improve the original image given by the objective. Another attempt is described by ADAMS in the "*Micrographia Illustrata*," Ed. 4, 1771.

This combining of a pair of lenses to form a higher-power objective was introduced as the result of practical experience, and was not due to any theoretical considerations.

The first theoretical attempt to improve the objective appears to have been made by EULER, who calculated a system of lenses—a bi-convex and a meniscus—which should give least aberration; but no maker, so far as we know, constructed a lens to his formula. Early in the 19th century, Sir WILLIAM HERSCHEL calculated several systems consisting of pairs of lenses, and actually had such "doublets" constructed to his calculations; but the opticians of his time were not able to make the doublets to his formulae with sufficient accuracy—their performance depended upon both the accuracy with which the curvatures of very

small lenses could be worked, and upon the accuracy of the mounting of these lenses, *i.e.* upon their centering and their distance apart—so no striking results were obtained with the lenses.

It was WOLLASTON, in 1829, who first produced a " doublet " which was simple and could be constructed fairly easily, and in which the aberration was largely corrected.

Both PRITCHARD in England and CHEVALIER in France made " triplets " for the microscope on similar lines, which gave good results. In PRITCHARD's objective the lowest lens had the longest focal length and was separated from the other two. In 1832 HOLLAND described a " triplet," replacing the lower lens of the WOLLASTON " doublet " by two plano-convex lenses close together ; this, as ROSS pointed out, enabled the errors to be reduced still further. These lenses are the high-water mark of the non-achromatic magnifier, and they were far superior to the earlier achromatics. Similar combinations continued to be made by many firms throughout the 19th century.

Materials other than glass have been used for lenses ; *e.g.*, the early Dutch telescopes had quartz lenses. BORELLUS suggested moulded lenses of fish-glue. HOOKE made a number of lenses of similar materials. STEPHEN GRAY devised a microscope in which a drop of water in a small cylindrical hole in a metal plate acted as the lens. He also substituted fish-glue for the water. In another case he drilled a hole 1/20 in. in diameter in a brass plate 1/10 in. thick, and filled it with a tiny drop of water containing infusoria, allowing the water to project on each side beyond the hole as a portion of a sphere. Thus the focus was within the body of the water, and any infusoria which happened to come into this focal plane were seen strongly magnified. The crystalline lenses of small fishes and eels have also been used as magnifiers.

But the most important experiments were made with lenses worked from jewel stones, such as sapphire, garnet, ruby, topaz, and diamond. Such lenses should have a superiority over glass lenses of equal power, as owing to their higher refractive index their curvature will be less, and therefore their aberration should be less. Also their dispersive power is less, so that the chromatic errors should also be less. It was found, however, that, apart from the difficulty of manufacture, such lenses suffered from defects due to the material. All crystals which belong to any but the cubic system are doubly refracting, and unless cut exactly perpendicular to an optic axis, will give a double image. Garnet and diamond crystallize in the cubic system, and therefore should not suffer from this defect ; but due to a want of homogeneity even these stones did not always give a satisfactory image.

The rapid improvements in the construction of the achromatic objective

at about this time, however, put an end to the attempts to make jewel lenses.

Later chapters deal with the early compound microscopes of CHÉRUBIN (1671), GRINDL (1687), BONANNI (1691), and MANN; with the microscopes of MARSHALL (1693), CULPEPER (1725-1730); the box microscope (1740); the CUFF (1744) microscope and its later modifications, and the still more highly complicated patterns of MARTIN (1704-1782), ADAMS (1747), and WATKINS (1754).

C. A. KOFOID.